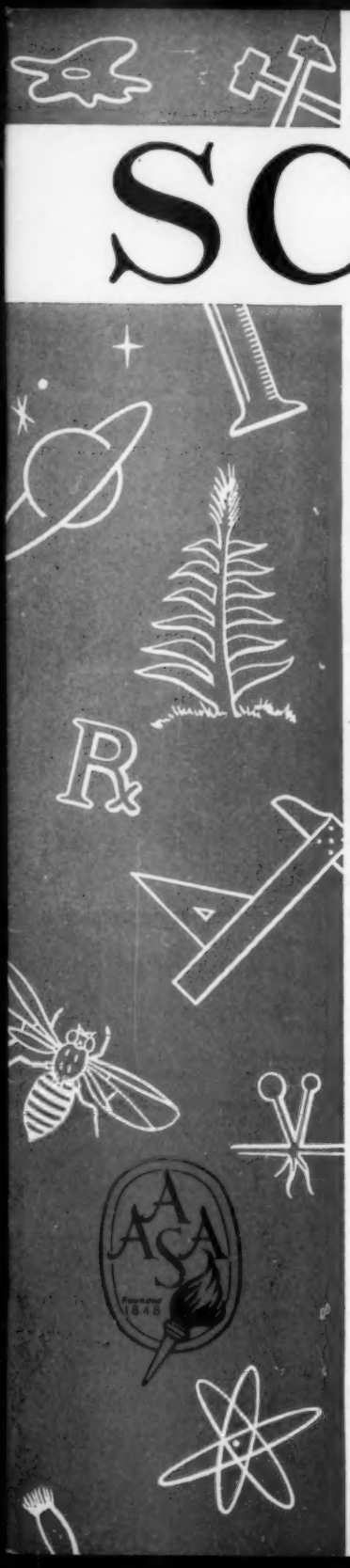

SCIENCE

15 OCTOBER 1954

VOLUME 120 NUMBER 3120

The Fungi as Aids in the Taxonomy of the Flowering Plants: *D. B. O. Savile* 583

Revised Symbols for the New Unstable Particles: *R. W. Thompson* 585

James Rollin Slonaker, a Worker in Vision, Nutrition, and Activity: *J. P. Baumberger* 587

News and Notes

Amino Acid Metabolism; and Regular Departments 589

Book Reviews

Tropical Meteorology; Geometrical Mechanics and De Broglie Waves; Fundamentals of the Working of Metals; Compounds with Condensed Thiophene Rings; The Actinide Elements; Calculations of Analytical Chemistry; Animal Breeding; Fresh Water from the Ocean; Titanium and Titanium Alloys; Biological Effects of External X and Gamma Radiation; Advances in Cancer Research 600

Technical Papers

Hemoglobin E, a Hereditary Abnormality of Human Hemoglobin: *A. I. Chernoff, V. Minnich, S. Chongchareonsuk* 605

Experimental Production of Renal Glycosuria, Phosphaturia, and Aminoaciduria by Injection of Maleic Acid: *H. E. Harrison* and *H. C. Harrison* 606

Influence of Hydrogen Ion Concentration on Radiation Effects: *T. Alper* and *M. Ebert* 608

Theoretical Rate of Fat Formation by Yeasts: *M. P. Steinberg* and *Z. J. Ordal* 609

The Fertile Life of Mouse and Rat Eggs: *A. W. H. Braden* and *C. R. Austin* 610

Nonidentity between Pepsitensin and Hypertensin Revealed by Paper Electrophoresis: *A. C. M. Paiva, T. Bandiera, J. L. Prado* 611

A Method of Assessing Experimental Pulmonary Edema: *M. H. Joffe* 612

Communications

Note on a Displaced Dogfish: *C. W. Hart, Jr.* 614

Word Saving, Good and Bad: Discussions by *F. C. Calkins* and *H. R. Struck* 614

The Explosion of a Planet: *P. S. Palmer* 616

Some Comments on a New University: Discussions by *P. H. Kopper* and *W. Seifriz* 617

Spurious Publication Dates 7A

Meetings & Conferences 12A

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Berkeley, California, December 26-31, 1954

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Spurious Publication Dates

SINCE it is established practice in scientific publications to give priority for reporting of scientific discoveries in accordance with the publication date of the journal carrying the report, the practice of some journals of placing a date on their issues that differs materially from the date of actual appearance becomes of importance.

Many of our American journals are received during the stated month of issue, but some we never get until 2 to 3 months after the date. Similarly, we receive European journals, many in the month following publication date, one weekly arriving regularly 17 to 20 days after publication, but some never arriving less than 2 to 4 months after date.

In both of these sets of late arrivals, which are consistent for certain journals, we are forced to conclude that the date imprinted on these journals is not the date of actual publication but rather perhaps the date of transit from the editor to the printer?

Should we regard a report in one of these journals appearing with a date in December of the previous year but not received until March as publication prior to a report in another journal bearing a January or February date and actually received during January or February? I think not. Yet the index compilers will list the papers one in one year and the other in the next.

Perhaps dates of receipts for publication would be a better way of appraising priority. But here other difficulties present themselves. As any editor knows, not all papers are ready for publication when received, and much time may elapse between receipt and sending to the printer, for various reasons. Hence papers in press are not available to readers for intervals varying from one week or so to more than a year. Papers "in press" are without influence on the progress of research in other institutions. Moreover, many journals do not print these dates of receipt. And, further, dates of receipt are ignored by the indexes.

The actual publication date should govern priority of publication, but it should be the real date of appearance and not a date 2 or 3 months earlier.

It is therefore urged that journals print on each issue the actual date or month of issue and that the date of receipt be included with each paper printed.

R. D. LILLIE

National Institutes of Health,
Bethesda, Maryland

Dr. Lillie is editor of the Journal of Histochemistry and Cytochemistry *and the author of the recently published* Histopathologic Technic and Practical Histochemistry *(Blakiston, New York, 1954).*

For Science, *the imprinted date is the date of publication and not, for instance, the date of transit from the editorial office to the printer. As for the date appearing at the end of each technical paper or communication, this is the date of receipt of the manuscript in the editorial office. If the manuscript must be returned to the author for substantial revision, the date of receipt of the revised copy is made the received date for the article.*

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SCIENCE is indexed in the *Reader's Guide to Periodical Literature.*
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The Fungi as Aids in the Taxonomy of the Flowering Plants

D. B. O. Savile

Botany and Plant Pathology Division, Science Service, Department of Agriculture, Ottawa, Ontario, Canada

THIS paper (*1*) is based on one prepared, by invitation, for the Sixth International Congress of Microbiology, and read there by proxy. As was indicated in the original text the invitation was, in truth, somewhat premature, for the development of techniques for using data from the parasitic fungi to reveal relationships in the flowering plants is still in its infancy and will require several years' elaboration before its full value can be assessed. Nevertheless, a few examples of the results obtained may be of general interest and serve to illustrate the methods involved.

Taxonomic mycology is a younger science than taxonomy of the flowering plants. Its development for a long period awaited improvements in the microscope. Even today its followers are fewer than those of phanerogamic botany, although the examination and comparison of microscopic specimens is more time-consuming than that of the flowering plants. Inevitably mycology lagged behind phanerogamic botany and accepted its dictates for a long period, although it has made great strides in recent years. For many years, mycologists concerned with evolution in the rust fungi tried to arrange them in chronological sequences based upon host relationship, relying, as was natural, on the older discipline. This method gave some initial assistance, for it was safe to assume that the rusts of ferns and conifers were in general more ancient than those of the flowering plants. Thus, if one could detect a continuous series of forms, it was sometimes possible to decide the direction of evolution. Attempts to refine the method, notably by Cunningham (*2*), met with scant success, because the host plants were generally arranged in the sequence of Engler and Prantl, which is now known to have several serious defects. Although we cannot yet present even an approximately complete phylogenetic diagram of the rusts, our understanding of them has progressed greatly, and the evolutionary trends within some genera are now well understood.

The difficulties that beset the student of the simpler, microscopic plants are occasionally compensated for by the fact that their simplicity strips them of a mass of confusing details and makes evolutionary sequences relatively easy to detect. Thus, as mycology matures, it may well be able to give more aid to phanerogamic botany than the science of phanerogamic botany formerly gave to mycology.

The examples discussed here fall into two categories, although additional studies may be expected to provide intergradation. First, there are detailed studies of small natural groups of parasites that may demonstrate relationship or relative age within a family or even a single genus of host plants. Second, there are broad, more or less statistical studies that may assist us in arranging the major groups of flowering plants.

Parasite relationship, if properly used, is a reliable biochemical tool; but obviously its value depends upon the achievement of a natural classification of the parasites. The responsibility for its use rests squarely with the mycologist. In the detailed study of a small group of parasites, we must base our classification on the most precise morphological study possible and attempt to find evolutionary trends, through developmental studies and by the demonstration of developing dispersal mechanisms or other valuable characters that will make plain the direction of evolution within a series. The details of host relationship are best ignored until the study of the parasites has been completed; otherwise one may disregard, through bias, apparent anomalies that may be vitally important. Reliable determination of the hosts is, of course, essential; and for this reason, if for no other, such studies must include only specimens personally examined and not literature records. Even when all these requirements are met, there is no assurance that parasite relationship will throw much light upon the phylogeny of the hosts. It will sometimes indicate the relative ages of the hosts without showing the evolutionary lines. Occasionally it will be of no assistance at all.

The second category of host-parasite study, that of broad general relationships, needs little explanation and is illustrated by the final example.

Phylogeny of Carex *and related genera.* The vast genus *Carex* and the smaller genera *Kobresia, Schoenoxiphium,* and *Uncinia* form a natural subfamily of the sedges (family Cyperaceae). In such a complex, the evolutionary clues tend to be hidden in a welter of confusing detail. Largely as the result of studies of some of the smuts that attack these plants, but also, of course, using more orthodox data, it has been possible (*3*) to achieve an improved arrangement for much of this complex, in particular in showing the existence and tracing the evolution of a strikingly natural subgenus of *Carex* hitherto unrecognized. The degree of success achieved was due to several favorable circumstances. An unusually long series of specimens was available (*4*); of these a high proportion was backed by adequate host specimens, whereby errors in host determination were reduced to a minimum. Most important of all, it chanced that species

differentiation in the smut genus *Cintractia* took place relatively late in the evolution of *Carex*.

The full significance of the latter circumstance was not realized until later, when I tried to use the North American rusts of *Carex* to secure further phylogenetic evidence. Sedges in various sections and often in two or three subgenera proved to be hosts for rust after rust. It was further found that, within each major host group, a rust tends to be restricted to a few closely related species. Evidently most species of rusts that attack *Carex* became differentiated very early in the evolution of the host genus. As the various subgenera and sections of *Carex* differentiated, races of the parasites developed on certain of them; but these races have since undergone little or no morphological differentiation.

Relationship of Sparganium *and* Acorus. The bur-reeds (*Sparganium*) and sweet flag (*Acorus*) are placed in the families Sparganiaceae and Araceae, respectively. In the system of Engler and Prantl these families were considered to be very widely separated, although some botanists, notably Hutchinson (*5, 6*), have believed them to be more closely related. The flowers of most species of *Sparganium* have a single style, but those of *S. eurycarpum* possess two styles. Because reduction in the number of styles or other flower parts is a well-recognized evolutionary trend, we consider that *S. eurycarpum* is the most primitive of the existing bur-reeds. The members of the family Araceae are characterized by a spikelike inflorescence usually partly surrounded by a spathe such as that of the skunk cabbage and jack-in-the-pulpit. In *Acorus* the spathe is simply a sword-shaped prolongation of the scape and might be considered either primitive or reduced.

It has long been realized that *Uromyces sparganii*, the rust that attacks *S. eurycarpum* but no other members of the genus, and *U. pyriformis*, which attacks *A. calamus*, are virtually indistinguishable. Recent studies (*7*) have shown that these rusts have a common host for the aecial stage and are not only morphologically identical but are cross-inoculable; that is, they are physiologically identical. A given race of a rust may not infrequently attack plants in closely related genera, but for one to atack plants in widely separated families would be astonishing. Examination, furthermore, shows striking similarity of the leaves and individual fruits, although not the whole inflorescence, of *A. calamus* to *S. eurycarpum* but much less to those of other *Sparganium* spp. The probable explanation is that *A. calamus* and *S. eurycarpum* are the most primitive representatives of their respective families, that the families are more closely related than even Hutchinson suggested, and that the two plants in question are more or less direct descendants of a common ancestor.

Taxonomic position of Allium. The genus *Allium*, comprising the onions, leeks, and so forth, has been placed by most authors in the family Liliaceae, but Hutchinson (*5*) considered that its affinities lay with Amaryllidaceae. Arthur (*8*) recorded that *Puccinia asparagi*, the rust of asparagus, which is an accepted member of Liliaceae, will slightly infect *A. cepa*, the common onion. This fact seems to offer some slight support for the retention of *Allium* in Liliaceae.

Relative age in Saxifragaceae. There are several short-cycled species of *Puccinia* that attack various genera of Saxifragaceae. It has recently been shown (*9*) that most of these rusts can be arranged in a reasonably clear evolutionary series. By plotting the host records of each rust, some idea can be obtained of the relative ages of the ten host genera concerned. Furthermore, three morphologically homogeneous rusts, *P. heucherae* var. *heucherae*, *P. heucherae* var. *austroberingiana*, and *P. heucherae* var. *saxifragae* occur on plants in five, four, and three genera, respectively. Thus we can tell the approximate relative ages of individual species in different genera. It is felt that, when accumulated phytogeographic data can be adequately interpreted, these clues from the rust relationships will allow a much clearer picture of evolution in this family.

Evolution of Puccinia *and relative age in the flowering plants.* Although a detailed picture cannot yet be presented, considerable progress has been made in our understanding of the large genus *Puccinia*. For our immediate purpose we may include *Uromyces*, having one-celled teliospores, with *Puccinia*, in which the teliospores are two-celled. *Puccinia* is one of the more modern genera of the family Pucciniaceae and is certainly much more modern than most genera of Melampsoraceae, universally accepted as the more primitive of the two families into which the rusts are usually divided. Nevertheless, *Puccinia* has a long developmental history, and in it there are many evolutionary lines. Although we cannot unravel all these lines, some of them are clear enough to demonstrate that one trend is common to most if not all.

In the most primitive species, the teliospores serve to overwinter the fungus and are the site of fusion of the paired haploid nuclei, but they do not serve as dispersal agents. They are firmly compacted in the sorus and have smooth walls, thickened at the apex, and strongly persistent pedicels. The more advanced species have teliospores with walls of nearly uniform thickness, often sculptured, and with deciduous pedicels. The latter spores are effective dispersal agents. In series after series, we can trace the evolution from the first to the second teliospore type, with ample evidence that evolution has actually been in this direction.

Examination of the North American species of *Puccinia* shows that nearly all attack monocotyledons or herbaceous dicotyledons. In contrast a high proportion of Melampsoraceae attack ferns and conifers, acknowledged as being in general older than the flowering plants, or woody dicotyledons. Most botanists agree that herbaceous plants have generally been derived from woody. Hutchinson (*10*), while supporting such a tendency, has suggested that, almost from their origin, the dicotyledons were split into two groups, one mainly woody and the other mainly herbaceous. Their rust relationships suggest that the herbaceous

dicotyledons are, in general, more modern than the woody members. Further support for this belief is found in the host relationships of the smut fungi and of the ascomycetous genus *Taphrina*. It appears probable that the separation seen by Hutchinson is chronological rather than genetic.

If we divide the species of *Puccinia* and *Uromyces* in Canada and the United States into three groups—primitive, intermediate, and advanced—on the basis of teliospore morphology and count the numbers that occur on (i) herbaceous dicotyledons, (ii) Glumiflorae (grasses, sedges, and rushes), and (iii) Liliaceae (*sensu lato*), Amaryllidaceae, and Iridaceae, we may hope to get some idea of the relative ages of these various plants. Such a division is shown in Table 1. The exact distribution must depend upon one's species concept and estimate of the latitude of each category, but the disposition is doubtful for only a few species. It is generally recognized today that the monocotyledons split off from the dicotyledons at an early stage when most members of the latter were woody. Thus the monocotyledons are all more modern than the most ancient dicotyledons, but it is not so clear how the more advanced members of each class compare.

Among the monocotyledons, the Glumiflorae represent one evolutionary line, adapted to wind pollination, and the Liliaceae and related families with showy flowers represent another line in which insect pollination has predominated. The figures in Table 1 strongly suggest that the Glumiflorae are, on the average, older than the herbaceous dicotyledons. The data for the Liliaceae, Amaryllidaceae, and Iridaceae are somewhat scanty, but these families appear to be more modern than the Glumiflorae. This finding may come as a surprise to many botanists, as it did to me, for we think of the reduced flowers of the grasses, sedges, and rushes as being highly advanced and thus modern. The explanation may lie in the fact that most Glumiflorae flower in the first or second year from seed, thus having a much shorter generation time than many of the lilies and their allies that do not flower until they are about 5 to 7 yr old. Thus evolution in the Glumiflorae may have proceeded very rapidly.

Table 1. Telial hosts of North American species of *Puccinia* and *Uramyces*.

Type of rust	Herbaceous dicotyledons	Glumiflorae	Liliaceae, Amaryllidaceae, and Iridaceae
Primitive	91	113	9
Intermediate	21	9	5
Advanced	168	3	21

Conclusion. These observations make only a trifling contribution to phanerogamic taxonomy in proportion to the many and serious phylogenetic problems that remain to be solved. Yet I feel that they are useful examples of the profitable results to be expected from more detailed studies of this sort. It must be repeated that the reliability of the method depends on the validity of the classification of the parasites; and thus the responsibility for its use falls fully on the mycologist.

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Revised Symbols for the New Unstable Particles*

R. W. Thompson

Department of Physics, Indiana University, Bloomington

ONE of the most interesting developments in physics in recent years has been the discovery of a surprising number of new heavy, unstable particles. There is no reason to think the list is yet complete; in fact, at the moment, the rate at which new particles are being found (about two per year) appears to be increasing as a result of concentrated effort in a large number of laboratories in many different countries.

The new particles are produced, directly or indirectly, in nuclear collisions of energy of the order of 1 Bev or more. For this reason, the new particles have been detected and studied primarily in cosmic radiation, although during the last year, artificial production has been achieved with the Brookhaven cosmotron. The methods of observation are quite varied; the principal experimental techniques are the nuclear emulsion, the magnetic cloud chamber, the multiple-plate

cloud chamber, and the hydrogen diffusion cloud chamber.

All the new particles at present identified are unstable, with lifetimes in the range 10^{-8} to 10^{-10} sec, disintegrating spontaneously into two or more lighter particles with a large release of energy. The charged parent particles are sometimes observed to decay in flight and sometimes to decay at rest at the end of their range. The neutral parent particles are not decelerated by collision loss of energy, hence their decay must be observed in flight.

The masses of the new particles appear to fall into two fairly distinct intervals. The first grouping occurs about halfway between the π-meson mass of 273 m_e and the proton mass of 1836 m_e. The best established example in this group is the τ-meson of 967 m_e that disintegrates into 3 π-mesons. The second grouping occurs at masses from 20 to 40 percent greater than the proton mass. The best established example is probably the neutral particle, usually denoted V_1^0, of 2182 m_e that disintegrates into an ordinary proton and a negative π-meson. There is reason to believe that the two groups are fundamentally different. Particles in the first group are usually referred to as "heavy mesons," whereas particles in the second group are sometimes referred to as "excited nucleons."

The development of the notation for the new particles has closely paralleled work on the particles themselves. For example, the first evidence for the existence of the new unstable particles was the phenomenon called "forked tracks" observed in the magnetic cloud chamber. The forked tracks were of two types: one in which a track was observed to undergo an abrupt angular deflection in the gas of the chamber without visible recoil; and one in which two tracks of opposite curvature apparently originated at a common point in the gas, again without visible recoil. The evidence indicated that events of the first kind resulted from the spontaneous decay in flight of heavy charged particles into one charged and one or more neutral fragments. Similarly, events of the second kind were shown to result from the spontaneous decay in flight of heavy neutral particles into two charged and possibly one or more neutral fragments. The name "forked tracks" was later changed to "V-tracks." The particles which, on distintegration in flight, produced the V-tracks were known phenomenologically as "V-particles" in the absence of more detailed knowledge of their specific properties.

When more data on the neutral V-particles were accumulated, two types were distinguished according to the mass of the positive decay fragment. In the majority of cases, the positive fragment was found to have a mass near that of the proton; however, in some cases, the measured mass was found to be near that of the π^+-meson. The two types of neutral V-particles were variously designated by the symbols V_1^0 or V^0, and V_2^0 or V^0, respectively. Further evidence indicated the complexity of both groups, and additional subscripts, V_3^0 and V_4^0, were introduced, but diversity of usage persisted.

The notation for the charged particles provides similar illustrations. For example, charged heavy, unstable particles were observed to decay at the end of their range, ejecting a single charged fragment. In the multiple-plate cloud chamber they were called "S-particles," and in the nuclear emulsion they were called "κ-mesons." The Greek letter κ was later replaced by the Latin letter K as the symbol for the phenomenological class, the Greek letter κ became the specific symbol for the type of K-particle that decays into a μ-meson and two neutral fragments. It was recognized, of course, that the classes $V^{\pm}$, S, and K might intersect, in fact might even be identical; however, in view of the differences in observational technique and in the absence of more definite information, the phenomenological distinction was preserved.

Thus, the rapid advances in the field have required frequent introduction of new symbols to designate the particles and continual revision of the meanings of symbols already introduced. Different authors have denoted the same particle (or group of particles) with different symbols or have used the same symbol to designate different particles (or groups of particles). As a result, the number of new symbols to be found in the literature exceeds the number of new particles.

Recently, a revised nomenclature for the new particles has been proposed (*1*) as an outgrowth of discussions held during the International Cosmic Ray Congress at Bagnères-de-Bigorre, France, 6–12 July

Table 1. Specific symbols.

Mass category	Specific symbol	Decay scheme	Mass	Remarks
K-mesons	$\tau^{\pm}$	$\rightarrow 3\pi$	967 m_e	Well established
	κ	$\rightarrow \mu + \nu^0 + \nu^0$	$\sim$ 1000 m_e	Very probable
	χ	$\rightarrow \pi + \nu^0$	$\sim$ 1000 m_e	Probable
	θ^0	$\rightarrow \pi^{\pm} + (\pi^{\pm}$ or $\mu^{\pm})$	966 m_e	Very probable*
Y-particles	Λ^0	$\rightarrow p + \pi^-$	2182 m_e	Very probable*
	Λ^+	$\rightarrow n + \pi^+$	$\sim$ 2200 m_e	Indicated by recent experiments†
		$\rightarrow p + \pi^0$		

* It is suggested by some results that there are particles with this decay scheme but different Q-values. The proposal suggests that these could be designated with different subscripts.

† Λ^+ here refers to the positively charged counterpart of Λ^0.

1953 (*2*). The authors of the new proposal are E. Amaldi, C. D. Anderson, P. M. S. Blackett, W. B. Fretter, L. LePrince-Ringuet, B. Peters, C. F. Powell, G. D. Rochester, B. Rossi, and R. W. Thompson.

The proposed new nomenclature provides two types of *generic* symbols to indicate the classification according to mass and according to phenomenon of decay, in addition to the usual *specific* symbols or "Christian names" to designate each individual type of particle.

Mass Categories

There are three mass categories demarcated by the π-meson mass and the nucleon masses. This grouping is suggested empirically as discussed in the first part of this article and has been more or less anticipated in previous usage.

L-meson: Any particle with mass equal to or less than that of the π-meson. This category thus includes the π-meson, μ-meson, and any other lighter meson that may be discovered. The name "light meson" is suggested for particles in this group.

K-meson: Any particle with mass intermediate between that of the π-meson and the proton. Thus the τ-meson is an example of a K-meson. The name "heavy meson" is suggested for this group.

Y-particle: Any particle with mass intermediate between that of the neutron and the deuteron. (The proposal suggests that this definition might be revised if fundamental particles heavier than deuterons are discovered.) The name "hyperon" is suggested for particles in this group.

It should be noted that the mass categories defined here exclude the ordinary nucleons (neutron and proton).

Phenomenological Categories

The new phenomenological categories extend and make more precise the phenomenological distinction already in use (V-particles, S-particles).

V-event: Any phenomenon that can be interpreted as the decay in flight of a K-meson or a Y-particle. Subdivisions: V^0-event, decay of a neutral particle; $V^\pm$-event, decay of a charged particle.

S-event: Any phenomenon that can be interpreted as the decay at rest of a charged K-meson or Y-particle.

Specific Symbols

The distinction between generic symbols and specific symbols is emphasized by the use of Latin letters for the former and Greek letters for the latter. This usage of the Greek letters is then identical with most previously well-established usage (for example, γ, μ, ν, π). It is not proposed, however, to change the accepted symbols for the proton (p) and the neutron (n). It is proposed to use the capital Greek letters for hyperons in order to distinguish them from mesons and other particles.

The new particles that have been given specific symbols to date are listed in Table 1.

References and Notes

* Assisted by the U.S. Office of Ordnance Research and by a grant of the Frederick Gardner Cottrell fund of the Research Corporation.

1. *Physics Today* **6**, 24 (1953).
2. See the report on the Bagnères Congress by M. M. Shapiro, *Science* **118**, 701 (1953).

James Rollin Slonaker, a Worker in Vision, Nutrition, and Activity

THE death of James Rollin Slonaker in Palo Alto on 3 January 1954, at the age of 87, marked the end of a long career as a teacher and researcher at Stanford University.

Dr. Slonaker's interest in biology, which led him finally to a professorship in physiology, began early in life. He was born in Farmland, Indiana, on 17 June 1866, and as a young boy he studied the nesting habits of birds. He earned his way through school, then taught all grades in a one-room house, making the fires himself and trudging 4 miles each way from home. In 1889 he graduated from the Indiana State Normal School, at which time he became principal of the high school and superintendent of schools in Elroy, Wisconsin. After 2 years he entered the University of Wisconsin, and in 1893 he was awarded the B.S. degree; a fellowship in biology then enabled him to go to Clark University, where he received the Ph.D. degree in 1896.

At Clark University, Dr. Slonaker studied with Clifton H. Hodge, a noted naturalist and authority on the structure of the eye. Dr. Slonaker spent the first 5 years after receiving his doctorate as assistant professor of zoology at Indiana University. One of his most promising students there was George Daniel Shafer, who later joined him as a member of the Stanford physiology faculty where they were always the closest of friends.

The first paper published by Dr. Slonaker was entitled "A comparative study of the point of acute vision in the vertebrate" [*Am. Naturalist* (1 Jan. 1896)]; this paper was typical of a series of 11 papers on the comparative anatomy and physiology of the eye that he published before 1921. About half of these

papers were written while he was on the faculty of Indiana University. As a result of this interest in the vision of birds, a great friendship developed between Casey Albert Wood, a specialist in the subject, and Dr. Slonaker. Dr. Wood often visited him at Stanford.

Between Indiana and Stanford, he spent 2 years at the University of Chicago as research assistant and associate in neurology. In 1903 he came to Stanford as assistant professor of physiology. He was promoted to associate professor in 1925 and to professor in 1930. At Stanford, Dr. Slonaker was known as a meticulous, straightforward, and clear teacher, and he was noted for his patience in guiding young researchers.

A later major interest of Dr. Slonaker was the one that brought him much renown: the relation of diet to the activity of the albino rat. His 45 articles on this subject cover the period 1907–39 at Stanford. One of the principal measurements of activity that Dr. Slonaker made was the distance run by the albino rats. He developed a cylindrical wire cage that rotated on a central horizontal axle on which the food and nesting boxes were swung. The rat could run at will, causing the cage to rotate and operate a counting device. When the rat was tired, it could retire to the food or nesting boxes. Astonishing distances were run by the animals —as much as 38 miles in 24 hours. The greatest activity occurred at night, and it was modified by the sex of the animal, oestrous cycles, age, diet, and so on. These observations led to a tremendous research program involving about 50 rotating cages and a large colony of inbred albino rats under a carefully controlled nutritional regimen. The work was supported by research grants. A series of studies on the effects of high-protein diet was also made. As the importance of this study to an understanding of sexual phenomena became apparent, the National Research Council's Committee for Research on Sex Problems gave its support to the investigation, and a series of papers on "The effect of different amounts of sexual indulgence in the albino rat," as well as studies of the effect of high-protein diet on longevity, gestation, fecundity, and so on, were published in the *American Journal of Physiology*.

The inbred strain of albino rats widely known as the "Slonaker-Wistar strain" had its origin in a pair carried by Dr. Slonaker in the Pullman from the Wistar Institute in Baltimore to California.

The "Slonaker activity cage" became a common tool, and it is still used in physiological and psychological research. This piece of apparatus was in line with Dr. Slonaker's interest in the development of apparatus and the construction of it by his own hand. Many fine pieces of laboratory equipment were made on the beautiful jeweler's lathe that he built from scratch. He spent many hours in the basement shop of the Physiology Department, building apparatus and printing laboratory syllabuses with his own hand press. Because of his interest in equipment, Dr. Slonaker organized the School of Biological Sciences supply room and shop, which has been a great asset to the several departments involved.

The later years of Dr. Slonaker's active career were devoted to the completion of his researches on the activities of the albino rat, and the working up of his accumulated data extended several years beyond his retirement in 1931. His last published paper appeared in 1939 in the Stanford University Biological Sciences Series in the form of a 67-page article on "The effect of different percentages of protein in the diet of six generations of rats."

Dr. Slonaker was a member of Phi Rho Sigma, Sigma Xi, the California Academy of Sciences, the American Physiological Society, and the American Association for the Advancement of Science, and he was past president of the Western Society of Naturalists. Most of his slides on the nesting habits of birds and on the structure of the eye of birds, moles, and so forth, were given to the California Academy of Sciences.

Dr. Slonaker had several outside interests, one of which was a 30-acre apricot orchard at Los Altos, which he developed by his own work and supervision. He operated the necessary horse-drawn, and later tractor-drawn, implement himself, and he directed the groups of workers seasonally employed to handle the fruit.

Another interest was the collection of the records for "A history and genealogy of the Slonaker descendants in America since early 1700." The genealogy was a hobby of his for 25 years and finally, when printed in a 732-page book in 1941, included 1500 relatives.

On 24 March, 1897, he married Marion Estey Stratton in Worcester, Massachusetts. Mrs. Slonaker survives him and still lives in Palo Alto. There they raised their three children—Clifton, Emily, and Marion.

Each afternoon, promptly at 4 o'clock, Dr. Slonaker went to the Faculty Club where for many years he was treasurer. There he engaged in a round of tennis or a game of cards. He thus became acquainted with most of the faculty and was well liked by all for his sense of humor and his quiet dignity. Dr. Slonaker was a tall, slim, aquiline-featured man who retained to an advanced age much of his strength and all of his faculties, and even worked in his garden to the day of his death.

J. Percy Baumberger
Department of Physiology, Stanford University

News and Notes

Amino Acid Metabolism

A symposium on "Amino acid metabolism," the fifth in the series sponsored by the McCollum-Pratt Institute, was held at the Johns Hopkins University, 14–17 June. The first day of the symposium was devoted to a consideration of the general aspects of amino acid biosynthesis and utilization, following which more detailed discussions of the intermediary metabolism of individual amino acids took place.

The first session was opened by A. Meister, who presented a comprehensive summary of the various types of reactions that amino acids undergo, giving special consideration to the role and mechanism of transamination in amino acid interconversions. The general problem of amino acid transport was discussed by H. N. Christensen, together with the results of *in vitro* studies on the concentration of amino acids by ascites tumor cells. Investigations on the biosynthesis of certain microbial enzymes that are formed only in the presence of inducer substances were discussed by Speigelman. The evidence that he presented is consistent with the view that these proteins, at least in the special case examined, are derived almost entirely from the free amino acid pool. The presence of polypeptide intermediates in any appreciable concentration appears to be excluded. Of particular interest was the evidence that the biosynthesis of these enzymes occurs only during the simultaneous synthesis of nucleic acid. This discussion complemented that of Gale, who reviewed the research done in his laboratory on the synthesis of proteins from free amino acids by bacterial cell fragments. A most exciting aspect of this work was the discovery that protein synthesis in this *in vitro* system is stimulated strikingly by the addition either of a mixture of purine and pyrimidine bases or of a ribonucleic acid fraction isolated from the organism.

The second session, which dealt with the metabolism of glutamic acid, proline, ornithine, citrulline, and arginine, was opened by S. Ratner. He reported on various aspects of the urea cycle and considered in some detail the properties of the new intermediate, arginosuccinate, and the role of this key compound in nitrogen metabolism. The participation of N-acyl glutamic acid derivatives as coenzymes in the synthesis of citrulline from ornithine, CO_2, and ammonia, and the formation of compound X as an unidentified intermediate in this process, were subjects covered by S. Grisolia. In formal discussions that followed, E. L. Oginsky, H. Busch, M. Korzenovsky, and H. D. Slade presented evidence obtained independently that the enzymatic degradation of citrulline to ornithine, NH_3, and CO_2 is coupled with the phosphorylation of ADP to ATP.

Histidine, leucine, isoleucine, valine, and lysine intermediary metabolism was the subject of the third program. Results of studies with *Neurospora*, showing that various phosphate esters of imidazole derivatives are intermediates in the biosynthesis of histidine, were presented by B. Ames; the pathways of histidine degradation in animals and in some microorganisms were reviewed by H. Tabor. E. A. Adelberg summarized the evidence available from mutant studies and isotope experiments pertaining to the synthesis of leucine, isoleucine, and valine. Provocative theories on the biosynthesis of leucine were proposed by both Adelberg and S. Weinhouse. E. Work concluded the session with a paper on some comparative aspects of lycine metabolism, with particular reference to the role of diamino pimelic acid as an intermediate.

During the period devoted to sulfur-containing amino acids, J. Stekol reviewed the metabolism in these compounds and particularly emphasized the role of transmethylation in methionine metabolism. A general discussion of various other aspects of transmethylation reactions was included. The intermediary formation of cysteine-sulfinic and cysteic acids in the oxidation of cystein to sulfonyl pyruvate and the elucidation of the coenzyme requirement for the individual steps of the process were reported by T. Singer. Of special interest was the incidental report that a soluble succinic acid dehydrogenase preparation had been obtained as an outgrowth of this research.

In a session on the metabolism of glycine and serine, Weinhouse discussed the significance of acetic, glycolic, glyoxalic, and oxalic acids in the oxidation of glycine. A thorough review of research on the synthesis of serine from glycine and formaldehyde or formate was presented by S. Sakami. Highlighting this discussion was a summary of the interrelationship of various folic acid derivatives and their roles as coenzymes in serine biosynthesis and in one-carbon metabolism in general. C. S. Mackenzie outlined the pathway of N-methylglycine metabolism and presented evidence indicating that an active formaldehyde derivative is formed in the oxidation of the methyl groups of these substances. Brilliant studies on the condensation of glycine and a succinate derivative to form α-amino-β-ketoadipic acid, the subsequent conversion of the latter compound to δ-aminolevulinic acid, and finally the participation of these compounds in the biosynthesis of prophorins comprised D. Shemin's interesting paper. Based on the discovery of these new intermediates, Shemin postulated the existence of an intriguing new "succinate-glycine" cycle as an alternate pathway of oxidative metabolism.

The final session of the symposium was concerned with the metabolism of aromatic amino acids. B. Davis reviewed the evidence obtained from studies on *Escherichia coli* mutants and supplementary enzyme studies establishing the sequence of reactions involved in the synthesis of aromatic amino acids. Of particular interest was the recent discovery that prephenic acid is an intermediate in the conversion of shikimic acid

to phenylalanine. The steps involved in the enzymatic degradation of phenylalanine and tyrosine were presented by W. E. Knox. In the final paper of the symposium, A. Mehler discussed the biosynthesis and degradation of tryptophan.

The afore-mentioned reviews were all supplemented by formal discussions by other investigators, who presented the most recent developments in their laboratories. No less important were the well-chosen moderators of the sessions, I. C. Gunsalus, P. P. Cohen, H. K. Mitchell, Vincent Du Vigneaud, H. G. Wood, and R. Stanier, who directed the stimulating informal discussions after the scheduled papers.

W. D. McElroy and his associates are once again to be commended for their part in organizing this excellent symposium. The forthcoming published account of the proceedings, complete with a careful transcript of the informal discussions, will constitute an up-to-date and comprehensive source of information about the metabolism of amino acids.

E. R. STADTMAN
National Heart Institute, National Institutes of Health

Science News

On 4 Sept. in connection with the recent book entitled ***The Hydrogen Bomb—the Men, the Menace, and the Mechanism*** by James Shepley, chief of *Time* magazine's Washington bureau, and Clay Blair, Jr., *Time's* Defense Department correspondent, Norris E. Bradbury, director of the Los Alamos Scientific Laboratory, held his first press conference in 8 yr. A complete transcript of that conference, during which Dr. Bradbury charged "obvious falsehoods," has been issued, as has the statement from which the following material is abstracted.

In late 1945 a small group of courageous and loyal scientists and technicians undertook to continue the postwar operation of the Los Alamos Scientific Laboratory. . . . In the face of an uncertain future . . . [and despite the] job offers from universities and industry [that] poured in upon them, . . . they stayed and built a laboratory that developed *every successful thermonuclear weapon that exists today*. . . . These deeds earned for the Los Alamos Scientific Laboratory the only Presidential Citation ever awarded to any laboratory for its extraordinary success in the development of both fission and fusion weapons, and its contribution to the collective security of the Nation and the free world. . . .

Thermonuclear work never stopped. Thermonuclear work grew as the Laboratory grew. . . . Although the thermonuclear program is referred to in every program of the Laboratory from 1945 on, some statements are of particular significance.

In a top secret letter to the Atomic Energy Commission dated December 9, 1949, three months after the Russian explosion, the Laboratory stated over the signature of N. E. Bradbury: "We propose to *augment* to the greatest extent possible the effort devoted to research on the problem of attaining a nuclear reaction involving the light elements. The goal of this effort will be an experimental test. . . ."

At every stage from 1946 to the present time, the fission and fusion programs—both in basic research and in practical application—were pursued with the maximum appropriate emphasis, with care, with precision, and with success. . . . The imputation of disloyalty to that now large group of scientists and technicians who are fundamentally responsible for every nuclear weapon, fission and fusion, that the United States has in its stockpile, who are responsible for the atomic weapons leadership that this country presently enjoys, and who are dedicated to the continuance of this leadership, is a tragic, if not malevolent, thing. The motives behind these accusations of Los Alamos are unclear; their bases are faulty and irresponsible information necessarily obtained from those who do not and cannot know the classified facts. . . .

Gordon Dean, former chairman of the Atomic Energy Commission, has prepared a review of the new book for the *Bulletin of the Atomic Scientists* in which he says that "It is a horrifying combination of little knowledge, outright untruths, and questionable motives. It is also, to put it mildly, a case of very bad reporting." Dean states that "the shocking, almost frightening, fact is that only one of the 16 men consulted by President Truman had been interviewed by the authors. . . . If it [the book] is accepted as a true account of America's struggle to achieve a thermonuclear weapon, it will be nothing short of a tragedy."

I. I. Rabi, chairman of the general advisory commission of the AEC comments: "A sophomoric science fiction tale to be taken seriously only by a psychiatrist . . . [the person responsible] should have either his head or his motives examined."

Hans Bethe, discoverer of the theory of thermonuclear fusion, says, "A vicious book . . . to list the falsehoods would make a book in itself."

Many U.S. scientists today are profoundly disturbed. The impact of governmental support of science to an extent that would have seemed incredible prior to the war, the demands for national security, and the inescapable involvement in the political turmoils inside the country and abroad unite to create pressures that seem to threaten the very existence of the free give-and-take of scientific argument. Can science flourish, no matter how richly endowed, when the expression of an opinion on scientific grounds may, if unpopular at the time or proved wrong in the event, subject the speaker to loss of support, to public vilification, maybe even to ostracism? In a remarkably open and unprejudiced presentation, Theodore H. White has discussed the current state of U.S. science in two articles in the *Reporter* [14 and 23 Sept.; see also *Science* **120**, 555 (8 Oct. 1954)]. His title, **"U.S. science: the troubled quest"** is exceedingly apt. His information on the subject is amazingly wide, and will undoubtedly add to the personal knowledge of almost any of us on the subject. His conclusions ought to be read not only by all American scientists but by all citizens who are affected—as who is not?

Scientists know that their own work, the inner harmony of their minds, is deeply affected by the mood of the society that nourishes them.

And it is this mood that is most discouraging. For U.S. science, in conscious struggle, has lifted itself finally out of the old once-valid tradition of American pragmatism into the realm of fundamentals, of the deep, pervasive contemplation of truths and theories that answer the everlasting whys and hows. Partly by accident, partly by calculated wisdom, it has been ushered into more power and authority than the community of science in any other country. And here it is trapped for it cannot stand alone, inviolate, in the political turmoil of the nation. . . .

Yet science's relations to security are not narrowly military. The Americans are engaged in a race with the Russians, as long as the two states exist, for dominance of the world. The Russian state, whose very essence is method, police, and security, nurses a science of great effectiveness but little ingenuity. It is derivative, far more than American science has ever been derivative, of other men's theories and insights born elsewhere. It is a science umbilically linked to espionage and parasitism on other men's fundamental work. In the race with this kind of science, U.S. science must start already handicapped. The power and the advantage that American science can give to America depend solely on America's ability to let imagination rove freely or wildly, awkwardly or arrogantly, even foolishly, wherever the mind wants to go.

No scientist this correspondent has talked to has claimed immunity from the obligations of good citizenship that rest upon others. Nor does any believe that in a strife-roiled world science can ignore the requirements of loyalty, or the dictates of secrecy where the fashioning of weapons is involved. No scientist, they point out, has ever been guilty of such a breach of security as the publication of the Oppenheimer report itself. Science concedes the need of the government to protect itself; what alarms it is the spreading of the limited military concept of security to the point where the true security and welfare of the nation are hurt. And when security spills over from the original area of weapons technology, where it is legitimate, into areas of biology, medicine and pure research, into the condition and temper of free men's minds, then the vitality of American science is eroded and the nation's health is undermined.

It is with a sense of helplessness that most scientists rest their case. Somehow, they insist, rules and procedures must be set up to safeguard security without crushing the inner essence of science which is creation, no matter how it expresses itself. But this, they say, is a task not for science to solve. It is a task for statesmen.—B. G.

Archeologists are excavating the **palace of King Edwin of Anglo-Saxon Northumbria,** built near Wooler, England, about A.D. 620. The excavators are working from aerial photographs showing the outline of the site in a grainfield. They expect to find foundations of an ancient church, a theater, and a fort, as well as the palace.

Although still in the category of pilot-plant development, General Electric's new **Irrathene 101,** an irradiated type of polyethylene plastic, is being produced in tape form. Like the conventional product from which it sprang, the material is tough, moisture resistant, and chemically inert; but unlike ordinary polyethylene, the new product does not "melt" until temperatures in excess of 350° are reached. In addition, it has outstanding resistance to stress-cracking in the presence of a broad range of commonly used chemicals. Bottles and dishes of this material, for example, have been found to retain their shape during sterilization, if the dishes are strain free originally. Not only does the toughening process allow such containers to stand up under steam-sterilization, but it also permits their use for packaging and storing a wide variety of chemicals, pharmaceuticals, and biological fluids, including blood plasma.

New evidence that **pasteurized milk** is just as nutritious as raw milk and that milk's food value does not change with the seasons is reported in a recent issue of the American Chemical Society's *Journal of Agricultural and Food Chemistry* by Conrad A. Elvehjem, chairman of the University of Wisconsin's department of biochemistry, and John N. Bixby, Arthur J. Bosch, and Arthur M. Swanson of the departments of biochemistry and dairy and food industries. Contradicting the results of some earlier experiments, the group's extensive tests with white rats show that neither pasteurizing nor homogenizing milk has an adverse effect on its nutritive qualities and that fresh milk is no less nourishing in winter than in summer.

The Atomic Energy Commission is advising public officials, stockmen, miners, and others in southwestern Utah and southern Nevada that it is preparing its Nevada Proving Ground for a series of **atomic tests** commencing early in 1955, probably about mid-February. The series will conform generally with those previously conducted in Nevada, including participation and support by the Department of Defense and Federal Civil Defense Administration.

The Royal Australian Air Force and the Commonwealth Scientific Industrial Research Organization will cooperate with the United States Navy in **rainmaking experiments** over Hawaii during the next few months. Hawaii is a natural laboratory for the study of warm clouds, which also occur almost daily in southeastern Australia. Tests in Australia have established that dropping dry ice into high cumulus clouds and spraying water droplets into the base of warm clouds will induce rain, but individual clouds must be treated and this is too expensive.

Another cloud investigation program is being conducted by the Air Force Cambridge Research Center of the Air Research and Development Command over the waters adjacent to Puerto Rico. Aircraft specially equipped with meteorologic sensing devices and modified radar will study the natural behavior of the cumulus clouds that are so prevalent in the area. A complete history of the life cycle of such clouds will be obtained, and the processes that lead to cloud growth, dissipation, and precipitation will be studied in detail.

A long-range research project for the chemical industry to provide readily accessible uniform data on the **physical properties of chemical compounds** has been announced jointly by the Carnegie Institute of Tech-

nology and the Manufacturing Chemists Association. The work will be directed by Frederick D. Rossini, head of the department of chemistry at the institute. Funds for the project will be contributed by the chemical industry, and the estimated $40,000 required for the first year of operation already has been subscribed.

A device to determine the **degree of pollution** of rivers has been developed at the Academy of Natural Sciences in Philadelphia. The instrument, believed to be the first of its kind, is about 2 ft long and consists of a series of glass slides on a Plexiglas frame supported by two floats. It can be used by industries to check the streams into which they discharge waste material.

The **nation's military research and development** program, together with the portions of the atomic energy program related to military use, now constitutes about half the research and development effort of the country. Donald A. Quarles, assistant secretary of defense for research and development, in making this estimate explained that the support provided by Congress for new weapons development and other investigations for military purposes is about $1.2 billions. This continues the peak level established during the Korean war for this purpose.

Scientists in the News

Northwestern University has announced the retirement, as professor emeritus of applied science, of **Paul E. Klopsteg,** director of research at the university's technological institute. Since 1951 he has been associate director of the National Science Foundation; during this period he has served as an adviser to Northwestern. Klopsteg will continue as chairman of the National Research Council's committee on artificial limbs, a post he has held since 1945.

Before joining Northwestern's faculty in 1944, Klopsteg taught at the University of Minnesota and held several posts in private industry. During World War I he was a development engineer with the U.S. Army ordnance department. He was a member of the board of governors of Argonne National Laboratory from 1947 to 1950, and in 1949–50 served as chairman of the board. In 1950 he was appointed a member of the security panel of the Atomic Energy Commission, and he is now a member of the commission's security review board. He is also a AAAS board member.

Author of several books and numerous articles, Klopsteg is past president of the American Association of Physics Teachers and of the Physics Club of Chicago. For 30 yr he was a member of the executive committee of the American Institute of Physics, and his 7 yr as chairman of that group encompassed the period of World War II.

Especially noted for his postwar achievements in the development of artificial limbs, Klopsteg in 1948 was awarded the medal of merit with Presidential citation for his work in that field and for service during World War II as chief of a division of the National Defense Research Committee and in other important governmental capacities. Other awards include the University of Minnesota outstanding achievement medal and the Modern Pioneers' award of the National Association of Manufacturers. He has honorary degrees from Northwestern and Wesleyan universities.

Donald L. Benedict, former assistant director of the engineering division at Stanford Research Institute, has been appointed director of physical sciences research. He will supervise all project work and institute-sponsored research in chemistry and chemical engineering, metallurgy, ceramics, biochemistry and physics.

In England, **Standish Masterman,** formerly assistant director of the Supply Ministry's guided missile research program, has been transferred to a civil engineering job in the ministry because of the discovery that he once was a Communist.

On 30 Sept. the 1954 Albert Lasker awards of the American Public Health Association were presented to **Leona Baumgartner,** New York City health commissioner, best known for her improvement of health conditions for new babies and their mothers as well as children generally; **John F. Enders** of Harvard Medical School whose work is contributing to the conquest of mumps, measles, and polio; **Edwin B. Astwood** of Tufts Medical College, for the medical control of overactive thyroids; jointly to **Alfred Blalock** and **Helen B. Taussig,** Johns Hopkins University, and **Robert E. Gross,** Harvard Medical School, well known for "blue baby" and "ductus" operations to correct congenital heart defects; and, in a group award, to the **Streptococcal Disease Laboratory,** Armed Forces Epidemiological Board, Francis E. Warren Air Force Base, Cheyenne, Wyo., under the directorship of Charles H. Rammelkamp, Jr., of Western Reserve University.

Fred W. Stewart of the Memorial Center for Cancer and Allied Diseases, New York, will deliver the fifth Augustus B. Wadsworth lecture in the Division of Laboratories and Research, New York State Department of Health, Albany, on 28 Oct. He will speak on "Wadsworth and Ewing: problems that would interest them today."

Pol Duwez, professor of mechanical engineering at California Institute of Technology, has terminated his association with the Jet Propulsion Laboratory and will devote all his time to teaching and research at the institute.

George S. Field of Ottawa, physicist, chief of Division "A" for the Canadian Defence Research Board, and scientific adviser to the chief of the Naval Staff, has been selected to serve in the United Kingdom as scientific adviser to the Air Ministry.

Sloan E. Jones, entomologist and former branch manager and consultant for a southwestern agricultural chemical company, has been named head of the U.S. Department of Agriculture's pink bollworm research laboratory at Brownsville, Tex. **A. J. Chapman,** a USDA entomologist, will be assistant head of the laboratory.

I. Frank Tullis, Memphis specialist in internal medicine, has been appointed to succeed the late Conley Hall Sanford as professor of medicine and chief of the division of medicine at the University of Tennessee College of Medicine.

Ernest Bueding, former associate professor of pharmacology at Western Reserve University, has assumed his duties as professor and head of the department of pharmacology at Louisiana State University. His work will include research on enzymes in relation to drug action.

Two executives of the Westinghouse Electric Corp.'s atomic power division, **John W. Simpson** and **John T. Stiefel,** have been appointed project managers in a realignment of the organization. Simpson becomes manager of the pressurized water reactor project, the reactor for the nation's first atomic powered electric generating station to be built at Shippingport, Pa. Westinghouse is building this reactor for the Atomic Energy Commission; the electric generating portion of the contract plant will be fulfilled by the Duquesne Light Co.

Stiefel becomes manager of the submarine thermal reactor project, which includes the building of the atomic engine for the first atomic submarine U.S.S. *Nautilus* and the land-based prototype of this engine that is now in operation at the Atomic Energy Commission's Reactor Testing Station in Idaho.

Jerome S. Horton, who has been in charge of ecologic and soil-moisture studies at the San Dimas Experimental Forest of the California Forest and Range Experiment Station near Glendora, has succeeded **Howard W. Lull** as officer in charge of the Vicksburg Infiltration Project of the U.S. Forest Service's Southern Forest Experiment Station. Lull has been transferred to the Northeastern Forest Experiment Station, Upper Darby, Pa., where he will be chief of the division of watershed management research.

Theodore Rasmussen, until recently professor of neurological surgery at the University of Chicago, has joined the staff of McGill University and the Montreal Neurological Institute as neurosurgeon and professor of neurology and neurosurgery.

Walter E. Loomis, professor of plant physiology at Iowa State College, has returned from a stay in Mexico where he served as a consultant on weed control problems with the Rockefeller Foundation and also studied coffee production methods.

Sir Alexander R. Todd, professor of organic chemistry at the University of Cambridge, England, has accepted an appointment as Arthur D. Little visiting professor of chemistry at Massachusetts Institute of Technology for the current semester. He will deliver a series of 20 lectures on "Selected topics in natural product chemistry."

At the 57th annual meeting of the American Society for Testing Materials **Sam Tour,** president of Sam Tour and Co., Inc., was honored by an award of merit "for long and fruitful service to the Society extending over many technical fields and administrative phases, for work on test methods, and especially for contributions to the metals and corrosion fields." Tour's participation in ASTM's activities started 33 yr ago.

Lloyd Russell Newhouser, a captain in the U.S. Navy whose work in plasma fractionation and whole blood distribution in World War II made him nationally known, retired last week after more than 30 yr of service. He goes to Miami, Fla., to head the Dade County Blood Bank. **Cecil L. Andrews,** also a captain, succeeds him as director of the professional division in the Bureau of Medicine and Surgery.

William H. Adolph has recently returned to this country, after 3 yr as professor of nutrition at the School of Medicine of American University of Beirut, Lebanon, where he organized a nutrition research unit. Plans are now under way to expand this laboratory to serve a larger Near East area. The new nutrition unit is to be affiliated with both the School of Agriculture and the School of Medicine of American University. Adolph was formerly associated with Yenching University and the Peking Union Medical College and has had a prominent part in organizing nutrition research and teaching programs overseas.

A portrait of the late **Aldo Leopold,** naturalist and wildlife expert and professor of wildlife management at the University of Wisconsin, was presented to the university during the recent meeting in Madison of the American Ornithologists Union. The portrait was comissioned by 27 of Leopold's former students and by Mrs. Leopold.

Walter J. Reppe, former vice president and director of I. G. Farbenindustrie A.G., and now research director of Badische Anilinund Soda Fabrik, West Germany, has arrived in this country to serve as Koppers visiting professor of chemistry at Carnegie Institute of Technology, under a grant from the Koppers Co. Reppe is well known for his development of acetylene processing methods.

Leonard J. Ortino, for the past 4 yr project engineer for International Business Machines at Poughkeepsie, N.Y., has joined the special products department of the Beckman division, Beckman Instruments, Inc., as chief mechanical engineer.

Theodor Philipp Haas, botanist at the Philadelphia College of Pharmacy and Science, has recently returned from a visit to Cuba, Costa Rica, and Panama where he studied tropical plants.

Paul A. Smith has retired from the Coast and Geodetic Survey, U.S. Department of Commerce, after an association of 30 yr. Prior to retirement he was on a special assignment to the Assistant Secretary of Defense for Research and Development. Smith was graduated with a degree in civil engineering from the University of Michigan in 1924 and joined the Survey that same year. During his career he has performed various assignments in hydrographic and geodetic surveying in the interior and along the coasts of the United States, Alaska, and the Philippine Islands, progressing through the various ranks until he reached that of captain. He served for an extended period in the Washington office as chief of the aeronautical chart branch and assistant to the director of the Survey. He has held various special assignments, including technical advisor, Department of Justice, Mare Island Litigation; delegate of the U.S. Government and of the National Academy of Sciences to the International Geographical Congress, Amsterdam; chairman of the aeronautical charts subcommittee of the Air Coordinating Committee; and chairman of the Department of Commerce Science Committee.

Smith was a leader in establishing the International Civil Aviation Organization. In this connection he served as U.S. representative on the council of ICAO, with the temporary rank of rear admiral and personal rank of minister, heading the U.S. Mission to ICAO. He held other important assignments in this field over a period of 9 yr, including delegate to seven sessions of the annual assembly of ICAO, and member of various committees and working groups of ICAO.

Among his outstanding accomplishments are the introduction of plastic sheets in cartography; development, with the U.S. Air Force, of plans and specifications for world aeronautical charts; cooperation with industry in the development of fluorescent paper for aeronautical charts; research on submarine canyons off the Atlantic Coast of North America and the Congo; the investigation of the topographic character of the submarine relief of the Pacific coastal areas, and also the navigational values of the application of topographic contouring to hydrographic surveys and nautical charts. In addition, he made contributions to the development of principles for the propagation of sound in sea water and the practical application of these principles to hydrographic surveying.

Smith was awarded the 1954 Department of Commerce gold medal for outstanding service. He is a recipient of the Washington Academy of Science award and a citation by the University of Michigan for outstanding contributions to the field of engineering. He is well known for his papers on hydrographic and geodetic surveying, cartography, submarine physiography, transmission of sound in sea water, and international civil aviation.

Warfield Garson, who recently received an M.P.H. degree from the Johns Hopkins University School of Hygiene and Public Health, has been appointed chief, venereal disease control section, State Board of Health, Raleigh, N.C. He replaces **W. G. Simpson,** now a Public Health Service regional consultant in the Dallas, Tex., office.

Benjamin F. Greene, Jr., an electronics engineer at the Air Force Cambridge Research Center, has been awarded a Department of the Air Force commendation for exceptional performance of duties. Greene led a team of specialists through 5 yr of secret work to develop the VOLSCAN system, a system of air traffic control that automatically selects and guides planes to landings at busy airports.

DeWitt Stetten, Jr., for several years chief of the division of nutrition and physiology of the Public Health Research Institute of New York, Inc., has been appointed associate director in charge of research at the National Institute of Arthritis and Metabolic Diseases, Bethesda, Md. Dr. Stetten will direct the institute's integrated program of fundamental research and clinical investigations into problems related to the several forms of arthritis and to such metabolic diseases as diabetes, vitamin deficiencies, gout, obesity, and disorders of the blood, bones, and liver.

Recent appointments to the research staff of the General Electric Research Laboratory, Schenectady, N.Y. are as follows: metallurgy, **David W. Lillie,** former chief of the metallurgy and materials branch of the U.S. Atomic Energy Commission's division of research in Washington, D.C., **William E. Tragert, Robert E. Keith,** and **Richard A. Swalin;** electron physics, **Raphael Littauer,** formerly of Cornell University, and **Wilbur Lakin;** chemistry, **Harold A. Dewhurst,** formerly of the Canadian Atomic Energy Project, and **George L. Gaines, Jr.;** mineralogy, **R. C. DeVries** of the U.S. Geological Survey.

Vaden W. Miles of the Wayne University physics department has accepted an appointment as visiting professor in general education in the physics department at Harvard University for the year 1954–55.

Richard Tredgold, former assistant lecturer in physics at the University of Nottingham, Nottingham, England, has been appointed research associate in the physics department of the University of Maryland, where he will serve as leader of the solid state theory research group under Ralph D. Myers, director of the group. The unit is supported by the Office of Scientific Research of the U.S. Air Force Air Research and Development Command.

Calvin L. Dickinson, American Potash and Chemical Corp. advisory engineer at Trona, Calif., has been named director of engineering there.

William W. Buechner, associate professor of physics at Massachusetts Institute of Technology in charge of the Office of Naval Research Van de Graaff Generator Group there, has been named a director of High Voltage Engineering Corp., New York. The firm makes supervoltage x-ray and other radiation producing machines used in medical therapy, industrial radiography, radiation sterilization, and in nuclear and industrial research.

Jerry W. Martin, former assistant professor of marketing and transportation and lecturer in management at the University of Texas, has become management consultant on the staff of Darell Boyd Harmon and Associates, Austin, Tex., industrial consultants in education, human performance, and research and design in the school field.

Samuel Richman, for 8 yr chief of radiological service at McGuire Veterans Hospital, Richmond, Va., has resigned to conduct a private practice in radiology in Greensboro, N.C.

The Illuminating Engineering Society has presented its 1954 gold medal to **Erwin F. Lowry** of Salem, Mass., manager of the lighting engineering laboratories of Sylvania Electric Products, Inc. Lowry received the honor for "making original and significant contributions to the development of the fluorescent lamp, especially in cathode design for gas filled tubes."

Charles J. Kensler, pharmacologist and biochemist, and former member of the faculty at Cornell University, has joined the staff of Arthur D. Little, Inc. He will organize and expand the biology department to offer increased research services in the field of pharmacology, biochemistry, toxicology, enzymology, nutrition, and industrial hygiene.

The British Institution of Radio Engineers has announced that **Rear-Admiral Sir Philip Clarke** is to be the next president of the institution.

W. Kenneth Clark has been appointed assistant medical and scientific director of the American Cancer Society. For the last 3 yr, as chief of the division of cancer control of the Pennsylvania Department of Health, Clark has worked to establish tumor clinics throughout the state to obtain statistics on cancer and to promote professional education among campaigners against the disease.

Meetings

The 5th annual meeting of the **Animal Care Panel** will be held 1–2 Dec. in Thorne Hall on the Chicago campus of Northwestern University. The program will be ready for distribution in October and may be obtained by writing to Robert J. Flynn, Secretary, Animal Care Panel, Box 299, Lemont, Ill.

The 64th meeting of the **Tennessee Academy of Science** will be held in Nashville on 26–27 Nov. at George Peabody College. Frederick T. Wolf of Vanderbilt University, vice president of the academy, is both general and program chairman, and C. S. Chadwick of Peabody is chairman of the arrangements committee.

"Public health problems in rural areas" was the subject for technical discussions at the **7th World Health Assembly.** Interest in the topic dates back to the Health Organization of the League of Nations. The assembly designated A. Stampar as the general chairman for the technical discussions and E. Braga, C. K. Lakshmanan, and J. Heng Liu as the three group chairmen. "Public health units in rural areas," "Rural sanitation," and "Zoonoses" respectively were chosen for separate discussions by each of the groups. An expert engaged by the secretariat and a corresponding member of the secretariat assisted at the sessions. The experts were F. Brockington, M. Petrik, and K. F. Meyer; members of the secretariat were C. K. Chu, R. N. Clark, and M. Kaplan.

The following documents were prepared to serve as a basis for consideration: "Background to rural health" by A. Stampar; "Development of health units in rural areas" by F. Brockington; "Rural sanitation" by M. Petrick; "Zoonoses in their relation to rural health" by K. F. Meyer; "Demographic and health statistics relating to urban and rural areas" by S. Swaroop. A selected bibliography on rural hygiene was also prepared by the secretariat.

Six sessions totaling 12¼ hr were allotted to the discussions. Over-all attendance was in excess of 100, and the average attendance at each meeting of the three groups was Health Units, 47; Sanitation, 21; and Zoonoses, 21.

Cleveland's Museum of Natural History, in collaboration with the Arctic Institute of North America, will sponsor a 3-day meeting in Cleveland during the second week of November. The purpose is to provide research people with information of **industry's needs in the Northlands.**

On the invitation of Otto Struve, and with the support of the National Science Foundation, an **Astronomy Teachers Conference** was held at the Leuschner Observatory, University of California, Berkeley, from 12 Aug. to 11 Sept. The purpose of the conference was to inform a group primarily interested in the teaching of astronomy of some of the recent advances in the field. The two principal lecturers were Bart J. Bok of the Harvard College Observatory, who spoke on the "Structure of our galaxy," and Armin J. Deutsch of the Mount Wilson and Palomar Observatories, who discussed "Problems of the sun and the physics of the stars." Peter van de Kamp of Sproul Observatory, Paul Herget of Cincinnati Observatory, and Struve gave shorter courses in their fields of interest. C. D. Shane, director of the Lick

Observatory, addressed the group on the "Distribution of the extragalactic nebulae." Edward Teller of the department of physics at Berkeley spoke on the "Origin of cosmic rays." In addition, 23 other participants, chiefly drawn from the Berkeley astronomical department and the Lick Observatory but including several conference members, spoke on a wide range of topics. There was a total of 57 lectures.

Several informal discussion periods were devoted to problems of teaching astronomy, elementary textbooks, and intermediate textbooks. It was generally agreed that strong efforts should be made to encourage qualified authors to write intermediate texts, especially on practical astronomy. Other activities of the conference included visits to the Lick Observatory, the Mount Wilson and Palomar Observatories, the Morrison Planetarium in San Francisco, and to the Radiation Laboratory on the Berkeley campus.

The National Science Foundation, the American Society of Biological Chemists, and the Division of Biological Chemistry of the American Chemical Society, acting jointly, will award individual grants to defray partial travel expenses for a limited number of scientists who will attend the 3rd **International Biochemical Congress** to be held in Brussels, Belgium, 1–6 Aug. 1955. Applications will be considered in two groups: those from scientists under 40 yr of age (about 20 grants); and those from more senior scientists (about 5 grants). Primary consideration in the selection will be scientific merit, but preference will be given to those who have not previously attended an international scientific congress or studied in Europe, and to those who are unable to attend without the aid of a grant. Application blanks may be obtained from the National Science Foundation, Washington 25, D.C. *Completed forms must be received by the Foundation before 3 Jan. 1955.*

The **British Association for the Advancement of Science** has announced that future annual meetings will be held in Bristol in 1955, in Sheffield in 1956, and in Dublin in 1957.

The 3rd **International Congress of Nutrition,** held under the auspices of the International Union of Nutritional Sciences, has recently concluded at Amsterdam. Over 360 delegates from 30 different countries attended, and some 67 communications were presented. The following officers were elected: hon. joint presidents, H. Dam (Copenhagen) and B. C. P. Jansen (Amsterdam); chairman, E. J. Bigwood (Brussels); sec.-gen., Leslie J. Harris (Cambridge, England). It was decided to hold the 4th International Congress in Paris in 1957.

A conference on **silicosis and occupational chest diseases** jointly sponsored by the McIntyre Research Foundation of Toronto, Canada, and the Saranac Laboratory of Saranac Lake, N.Y., has been scheduled for 7–9 Feb., 1955, at Saranac Lake. These two organizations have for many years been conducting research along parallel lines, and the papers to be read in the five sessions will all report on original work conducted or sponsored by them. In addition there will be presentations of guest lecturers.

Anthony J. Lanza, formerly director of the Institute of Industrial Medicine and now emeritus professor of industrial medicine at New York University-Bellevue Medical Center, will be chairman of the conference. Business arrangements and reservations will be handled by Norman R. Sturgis, Jr., of the Saranac Laboratory. Physicians, scientists, and businessmen concerned with problems of occupational chest diseases in all parts of the United States, Canada, and foreign countries are invited to attend.

The **American Psychosomatic Society** will hold its 12th annual meeting in Atlantic City, 4–5 May, 1955. This meeting will be immediately preceded by those of the American Society for Clinical Investigation and the Association of American Physicians, and will be followed by the meeting of the American Psychoanalytic Association.

The program will emphasize investigations in the theory and practice of psychosomatic medicine as applied to adults and children in all of the medical specialties, and contributions in psychophysiology and ecology. Abstracts for 20-min papers should be submitted in duplicate *by 1 Dec.* to the chairman of the program committee, Lawrence S. Kubie, 551 Madison Ave., New York 22.

The annual meeting of the **American Association of Physics Teachers** will be held 27–29 Jan. 1955 in conjunction with the annual meeting of the American Physical Society in New York. Columbia University is no longer able to provide for the societies, and arrangements are being made with hotels. Those wishing to present papers should send titles and abstracts to R. R. Palmer, Beloit College, Beloit, Wis., *before 1 Dec.* It is hoped to allow more than the traditional 10 min for some of the contributed papers when such additional time is requested.

The 4th annual **Eastern Joint Computer Conference and Exhibition,** sponsored by the American Institute of Electrical Engineers, the Institute of Radio Engineers, and the Association for Computing Machinery, will be held in Philadelphia, 8–10 Dec. The theme of the conference will be "Design and application of small digital computers." Information may be obtained from the Eastern Joint Computer Conference, P. O. Box 7825, Philadelphia 1.

The 2nd science congress of the **Pan Indian Ocean Science Association** was held at the University of Western Australia, Perth, 17–24 Aug., under the auspices of the Commonwealth Government of Australia and the Australian National Research Council, and under the leadership of H. J. Bhabha of India, president, and A. P. Elkin of Sydney, president-elect. It was attended by some 40 delegates representing Australia, Burma, Ceylon, France, India, Madagascar, Malaya,

Netherlands, Pakistan, and Portugal, and several representatives of scientific and research organizations, for example, UNESCO. There was a total registration of nearly 400 persons, including many students from overseas now studying in Australia under the Colombo Plan. J. W. Wells, professor of geology at Cornell University, attended as a guest representing the National Research Council. The meeting was an unqualified success from all standpoints, largely owing to the long planning by A. D. Rose, honorary secretary.

The regular sessions of the sections of physical sciences, biological sciences, geological sciences, agricultural sciences, economics, education, the social sciences, geography and oceanography, and human ecology, resulted in more than 200 papers and reports, many of them arising from interim investigations suggested at the 1st congress in Bangalore, India, in 1951. Many proposals were made for new or further studies to be reported on at the next congress, which will meet in Madagascar in 1957 with J. H. Millot as organizing secretary.

The objects of the association are (i) to discuss and promote concerted action in regard to scientific problems specially affecting the countries around the Indian Ocean, and to make recommendations to the countries concerned when necessary, and (ii) to strengthen the bonds of friendship among all the peoples in the Indian Ocean area.

Society Elections

Sigma Delta Epsilon: pres., Emily Wolff, Wellesley College; 1st v. pres., Elva Shipley Meyer, University of Wisconsin; 2nd v. pres., Irene Corey Diller, Institute for Cancer Research, Philadelphia; sec., Mary Gojdics, Barat College; treas., Teresa Cohen, Pennsylvania State University.

Society of Protozoologists: pres., Lowell E. Noland, University of Wisconsin; v. pres., Alfred M. Elliott, University of Michigan; treas., William F. Diller, University of Pennsylvania; sec., Norman D. Levine, University of Illinois.

Massachusetts Society for Research in Psychiatry: pres., Rudolph Kaldeck; v. pres. Daniel H. Funkenstein; sec.-treas., Max Rinkel.

American Society for Horticultural Science: pres., E. S. Haber, Iowa State College; v. pres., M. B. Davis, Central Experimental Farm, Ottawa; sec.-treas., Freeman S. Howlett, Ohio Agricultural Experiment Station, Wooster.

American Association of Colleges of Pharmacy: pres., Joseph B. Burt, University of Nebraska; v. pres., Chauncey I. Cooper, Howard University; sec.-treas., R. A. Deno, University of Michigan; pres.-elect, Linwood F. Tice, Philadelphia College of Pharmacy and Science.

American Microscopical Society: pres., Theodore L. Jahn, University of California, Los Angeles; 1st v. pres., Robert W. Pennak, University of Colorado; 2nd v. pres., Clifford O. Berg, Ohio Wesleyan University; sec., C. J. D. Brown, Montana State College.

The Combustion Institute: pres., Bernard Lewis; v. pres., H. C. Hottel; treas., Stewart Way; sec., Glenn C. Williams.

Education

The 14th annual **Science Talent Search,** conducted by Science Service and supported by the Westinghouse Educational Foundation, was launched on 23 Sept. with an invitation to seniors in 27,000 public, private, and parochial schools throughout the country. Students will compete for 40 scholarships, totalling $11,000, and a 5-day visit to Washington. Honorable mention status will go to 260 others.

The **Polytechnic Institute of Brooklyn** and **New York University's College of Engineering** are celebrating centennials this year. Both organizations are planning special programs that will bring together scientists, engineers, and scholars from many parts of the world.

Ground-breaking ceremonies at the site of the new **Saint Mary's College Science Hall** (Winona, Minn.) were held on 17 Sept. The three-story building, which has an adjacent auditorium with a seating capacity of 235, is expected to be ready for occupancy in about a year.

An affiliation between the University of Pennsylvania School of Veterinary Medicine and the New York Women's League for Animals has been announced. A new **Institute for Veterinary Research** is to be set up in New York in association with the Ellin Prince Speyer Hospital, which the League has maintained for 40 yr. New construction is planned, and a board of scientific advisers has been appointed that will function under the chairmanship of Geoffrey W. Rake, research professor of microbiology in medicine in both the School of Medicine and the School of Veterinary Medicine of the University of Pennsylvania.

Konrad Z. Lorenz, departmental director, Max-Planck-Institut für Verhaltensphysiologie, Schloss Buldern über Dülmen, Westfalen, Germany, will deliver the three Edward K. Dunham lectures at the Harvard Medical School, Boston, on the general subject of the approach to behavior study from the viewpoint of comparative phylogenetics: 25 Oct., "Innate motor patterns"; 27 Oct., "Innate releasing mechanisms"; 29 Oct., "The organization of innate behavior." On 1 Nov. he will give a fourth lecture, "The problems of expression movements and ritualization," under the auspices of the Harvard departments of biology and psychology.

William Guild, a retired real estate man from New England and a hobby scientist, has set up an effective program for helping Florida school children understand the natural wonders around them. Believing that **encouragement at the grade-school level** is the way to solve the shortage of young people choosing scientific careers, Guild started the Science Center in a St. Petersburg junior high school. Through it youngsters learn the scientific approach—and its satisfactions—by studying such subjects as crabs, caterpillars, telephones, snakes, insects, and methods of crime detection.

The program aroused the interest of 170 teachers and their 6400 pupils. During the first year they completed 543 projects loaned by the center. At the first annual science fair in St. Petersburg this past spring, attended by 11,200 people, 65 percent of the 300 exhibits were from grade schools.

Through Guild's efforts, assistance has come to the center from many directions. The University of Florida supplied a large quantity of demonstration equipment; and the University of Miami, Florida State University, the State extension service, the local telephone company, the Florida State Museum, and the Florida State Geological Survey, all provided scientific material in the form of literature, exhibits, and so forth. Guild has largely financed the center himself; now he is looking for a foundation grant to develop a statewide organization. He hopes to bring "Science is Fun—See it—Touch it—Do it Yourself" to grade school pupils all over Florida and indeed throughout the United States.

Five years of study in the Massachusetts Institute of Technology department of **naval architecture and marine engineering** will lead, under a new course, to two degrees awarded simultaneously: the B.S. in naval architecture and marine engineering, and a new master's degree, the M.S. in shipping and shipbuilding management.

The University of Miami School of Medicine will sponsor a symposium on **Industrial Medicine,** 3–4 Dec. The meeting will be cosponsored by the Industrial Council of the American Medical Association, the American Academy of General Practice, and the Liberty Mutual Insurance Co. For information address the meeting chairman, William B. Deichmann, Department of Pharmacology, University of Miami School of Medicine, Coral Gables, Fla.

Funds for **Wayne University's new medical library** were assured with the recent announcement of a $175,000 grant from the Helen L. DeRoy Foundation. Named in honor of Mrs. DeRoy, the new structure will be built at the north end of the medical campus in downtown Detroit. The gray brick, one-story building will have 29,000 ft² of floor space and will be able to accommodate 150,000 volumes and hundreds of current periodicals.

Available Fellowships and Awards

The **Lalor Foundation,** through a grant to the **Marine Biological Laboratory,** Woods Hole, Mass., is offering a limited number of postdoctoral fellowships in biochemistry, biophysics, and physiology, designed primarily for young scientists desiring to work not less than two consecutive months during the summer on investigations for which the opportunities provided at Woods Hole are particularly appropriate. The stipend is intended to cover laboratory fees, travel, and living expenses. *Completed applications should be received by 15 Dec.* Further information may be secured from the Director, Marine Biological Laboratory, Woods Hole, Mass.

Attention is called to the fact that **Fulbright scholarships** for graduate study abroad are open to professional persons not now engaged in college or university study. Any U.S. citizen between the ages of 18 and 35 with a bachelor's degree is eligible for these awards. Applicants must be at the predoctoral level. *1 Nov. is the closing date* for applications for the 1955–56 academic year. Candidates-at-large may apply directly to the Institute of International Education, 1 E. 67 St., New York.

Grants and Fellowships Awarded

The **Upjohn Co.** has announced the following research grants:

University of Illinois. H. E. Carter, department of chemistry. Chemistry and preparation of antibiotics, $9000.
University of Illinois. D. Gottlieb, department of horticulture. Research on antibiotics, $6000.
University of California. J. Halstead. Vitamin B-12, $3000.

The **Harvard University School of Public Health** has announced the award of 14 postgraduate scholarships to prospective public health leaders. The scholarships are the first to be granted by the school since its inception 41 yr ago. The scholarship program grew from surveys which showed an acute lack of qualified public health specialists within the United States and in foreign countries.

The 14 persons, 8 women and 6 men, who have received the scholarships were selected from 74 applicants. The recipients and their former affiliations are J. R. DuBois, assistant supervising nurse, Saranac Lake, N.Y.; R. L. Bragg, Pine Ridge Indian Hospital, S.D.; O. Calabi, Harvard School of Public Health; T. T. Woo, Army Medical Corps, Ft. Belvoir, Va.; N. J. Wilson, director of health education, Upper Columbia Conference of Seventh-Day Adventists, Spokane, Wash.; E. Rose, public health nursing consultant, University of Pennsylvania; T. A. Montgomery, California State Department of Public Health; D. O. Jones, instructor, Ohio State University; P. Fry, who has been a Fulbright fellow in the Cook Islands; M. C. Egan, New York State Department of Health, B. Y. Akerren, Institute of Bacteriology,

Goteborg, Sweden; R. S. Young, Dental Clinic, Hong Kong and Kowloon Trades Council; J. M. Vasey, St. Thomas' Hospital, London; M. E. Thomas, Freetown, Sierra Leone, British West Africa.

In the Laboratories

Fabric Research Laboratories, Inc., Boston textile research and consulting organization has broken ground for a new 16,000 ft² laboratory and office building in Dedham, Mass. The organization engages in research, development and consultation for the textile, paper, leather, elastomer, and plastics industries. Its current staff numbers 40 scientists and engineers under the direction of Walter J. Hamburger.

In its potash mine in Carlsbad, N.M., the **Potash Co. of America** is extending the present conveyor system to a total length of approximately 7¼ mi, making it the longest conveyor system in the U.S. It will operate 1000 ft underground in a potash ore seam about 4 ft thick. Mining machines will extract the ore and deposit it on shuttle-type conveyors which automatically transfer it by intermediate belts to the main line or "mother" belts. The potash will then be carried more than 5 mi to a 3000-ton underground storage pocket from which it will be withdrawn as needed by a special rotary plow feeder and delivered to a vertical skip hoist by additional belt conveyors. The conveyor system when completed late in 1955 will consist of 45 units linked together to carry the material in a continuous flow from the mining area to the refinery.

The **Fairchild Engine and Airplane Corp.,** Hagerstown, Md., is planning a new main engine division plant and turbine test laboratory. The structure will have 400,000 ft² of working area covering more than 80 acres. The building will replace the Farmingdale factory now occupied by the Fairchild engine division, which was recently sold to the Republic Aviation Corp.

Miscellaneous

The American Association of Physics Teachers is undertaking the production of a book of ***Advanced Undergraduate Experiments in Physics,*** as a memorial to the late Lloyd William Taylor. Some 100 physicists have already contributed experiments and suggestions. Despite this fine response, there doubtless are other interesting and illuminating experiments that should be included in this book to give them wider circulation and thereby to enrich the instruction in advanced physics courses.

Although primary responsibility for the book belongs to A.A.P.T. members, contributions are cordially invited from all physicists and from colleagues in other sciences, outside the United States as well as inside. All fields of physics are represented. Informally written notes, or laboratory instruction as used by students, are quite satisfactory, since the editors must put all material into publishable form. To be of use this material must reach the editors *not later than 1 Mar., 1955.* It may be sent to T. B. Brown, The George Washington University, or to the appropriate section editors: *mechanics,* R. H. Bacon, 405 Bedford Rd., Pleasantville, N.Y.; *heat,* R. L. Weber, Pennsylvania State University; *acoustics,* L. R. Weber, Colorado State College; *electricity, magnetism, and electronics,* M. C. Harrington, Drew University; *optics,* H. A. Nye, Cornell University Aeronautical Laboratory, Buffalo, N.Y.; *atomic physics and spectra,* S. C. Brown, Massachusetts Institute of Technology; *radioactivity and nuclear physics,* R. R. Palmer, Beloit College.

The U.S. Civil Service Commission has announced a **clinical psychologist examination** for positions having salaries of from $5940 to $10,800 a year. Appropriate education and experience are required; no written test will be given. Applications will be accepted by the U.S. Civil Service Commission, Washington 25, D.C.

Necrology

Milton A. Cross, 79, mechanical engineer and inventor, Detroit, Mich., 25 Sept.; **David Fishkind,** 59, chemist and production manager of the Verona Chemical Co., Newark, N.J., 27 Sept.; **Roderick V. Grace,** former clinical professor of surgery at Columbia University, New York, 27 Sept.; **James F. Henegan,** 57, dentist, pioneer anesthesiologist, and lecturer, New York, 26 Sept.; **Irving H. Osborne,** 77, retired chief engineer for the Federal Shipbuilding and Dry Dock Co., Jersey City, N.J., 27 Sept.; **Edith M. Patch,** 78, author and retired head of the department of entomology at the University of Maine Agricultural Experiment Station, Orono, Me., 28 Sept.; **George W. Riley,** 87, pioneer osteopath and former president of the American and New York State Osteopathic Associations, Pittsburgh, Pa., 25 Sept.; **Walter F. Rittman,** 70, pioneer in the field of chemical engineering of petroleum and former head of the engineering department of Carnegie Institute of Technology, Pittsburgh, Pa., 26 Sept.; **George H. Shull,** 80, former president of the American Society of Naturalists, associate editor of *Genetics,* developer of hybrid corn, professor emeritus of botany and genetics at Princeton University, Princeton, N.J., 28 Sept.; **James E. Waddell,** 60, former electronics engineer for the Signal Corps and physicist at the Johns Hopkins University Laboratory of Applied Physics, Silver Spring, Md., 23 Sept.

Erratum. W. H. Price *et al.* [*Science* **120**, 457 (17 Sept. 1954)] inadvertently failed to state that the major part of the work described was carried out under contract No. DA-18-064-CML-2365 with the Chemical Corps, Biological Division, Camp Detrick.

Book Reviews

Tropical Meteorology. Herbert Riehl. McGraw-Hill, New York–London, 1954. x + 392 pp. Illus. $8.50.

About half the earth's atmosphere is contained in the tropics. Yet the literature dealing with tropical weather, while fairly extensive, has consisted of random papers or chapters in widely scattered publications and several books concerned with specialized subjects, such as hurricanes. The author has performed a great service for the meteorologic profession by gathering together and presenting in one volume a wealth of information on tropical meteorology. In doing this, he has permitted the influence of his own original and prolific papers to be quite evident. In a less able author this could be a severe drawback. In the present case, the result is a well-integrated account in which the theoretical and speculative aspects are remarkably free from attack. At the same time, Riehl has only rarely failed to consider the findings of other workers in the field.

The style of writing is clear and free flowing, and the presentation is well organized. The author begins with a synoptic-climatologic treatment of wind, pressure, temperature, rainfall, and cloudiness. He proceeds by discussing convection, mainly as it is concerned with low-latitude cloudiness, and then presents a chapter on the physics of tropical rain (prepared by his colleague, Raymond Wexler). Riehl touches on the subjects of weather observing and analysis, and then he moves briefly into the realm of theory with a short discussion of divergence and vorticity. He concludes with and places greatest emphasis on chapters dealing with low-latitude disturbances, for example, hurricanes and waves in the easterlies and how they fit into the general-circulation scheme. This is probably the most valuable portion of the book, and it is done convincingly. While studying it, the reader should nevertheless always be aware of the profound lack of low-latitude data from which one can generalize on the structure and behavior of tropical phenomena, and he should constantly make the effort to differentiate between fact and theory. The over-all treatment is empirical or synoptic rather than dynamic; the mathematics is simple and unobtrusive. The author has elected to explain a number of important and quite involved atmospheric phenomena and processes of the tropics, although it is true that he does not devote much space to the methods of forecasting them. Proper understanding of the explanations requires basic meteorologic training. Thus, the layman will derive benefit mainly from the climatologic and purely descriptive material. However, as a text or as reference material for the trained meteorologist concerned with the synoptic meteorology of low latitudes, this book admirably fills a long-felt need.

ROBERT D. FLETCHER

Scientific Services, Air Weather Service, MATS, Andrews Air Force Base, Washington, D.C.

Geometrical Mechanics and De Broglie Waves. J. L. Synge. Part of Cambridge Monographs on Mechanics and Applied Mathematics. G. K. Batchelor and H. Bondi, Eds. Cambridge Univ. Press, New York, 1954. vi + 167 pp. Illus. $4.75.

This small and well-made book has an unusual place among monographs on physics. It does not summarize a rapidly growing field, nor is it an exposition of a well-established branch of theory. Instead it is an original setting forth of a theoretical topic that seems to fill a logical gap in the structure of physical theory rather than to form the basis for new developments.

The Hamiltonian theory of the characteristic function (and the closely related eikonal) is both an elegant and a practical tool for studying the motion of rays or of particles. Its greatest usefulness has been in geometric optics, but the intimate relationship between the ray paths of optics and the trajectories of particles and between the wave fronts of optics and the surfaces of constant action in mechanics is familiar to students of dynamics and optics alike, especially since De Broglie. Synge has pressed the analogy a step ahead; he has introduced the analogue of Hamilton's characteristic function, and of the extremal principle that lies behind it, into the pseudo-Euclidean four-space of Minkowski. Proper time and not ordinary time now labels the motions.

From such a theory, developed quite generally and elegantly, but not without many examples of the heuristic arguments always so helpful to physicists, Synge has drawn a collection of interesting and somewhat recondite consequences. Typical is his result, quite familiar, which shows that the particle velocity is the group velocity of a packet of wave fronts. This comes without any introduction of wavelengths, frequency, or phases. Only surfaces of constant action, arbitrarily spaced, are used. Finally he is able, at least approximately, to introduce phase and wavelengths by marking off action intervals the size of Planck's constant. He can then make a subtly pleasing geometry appear out of familiar Schrödinger wave functions in a number of special examples.

The monograph presents the geometric optics of wave fronts for which the Schrödinger theory gives the true wave equation. Its wider applications are not evident, unless perhaps to such problems as the theory of solids where complicated one-body potentials are encountered, and any new approximation method may be worthwhile. But the work mainly has the appeal of all unifying and completing theories, and it is intrinsically of beauty and interest. Anyone who is quite familiar with Hamiltonian theory, and a little bit at home in Minkowski space, can find insight in the work if it strikes his fancy; but reading it without such a background cannot be recommended.

P. MORRISON

Department of Physics, Cornell University

Fundamentals of the Working of Metals. G. Sachs. Interscience, New York; Pergamon Press, London, 1954. vii + 158 pp. Illus. + plates. $4.75.

This interesting little book subdivides conveniently into two parts. Materials aspects of metalworking are treated in the first three chapters, comprising somewhat less than half of the book: "Effect of temperature and speed on forming"; "Relations between chemical composition, phase changes, and forming characteristics"; "Effects of grain structure on forming." The illustrations in this part are largely schematic and there are few data. The remaining three chapters are concerned with simple, qualitative analyses and discussions of metalworking theory and practice: "Some general concepts of metal forming"; "Basic types of forming methods"; "Progressive fabricating." On the whole, the second part is well illustrated, and there is considerable emphasis here on sheet-metal fabricating.

In view of its brevity and elementary aim, the book does not have much depth. The coverage, however, is broad. A consequence is that some parts may not be as clear as an unprepared reader would like. A section on "The general nature of phase changes," for example, covers only four pages. It also would have been helpful to include more precise definitions of such quantities as stress, strain, forming resistance, ductility, and so forth. The book would, therefore, be of somewhat limited value for teaching purposes and in processing research and development work. Perhaps the most valuable features are the organization and classification of phenomena and processes that it contains. Important concepts are set forth by means of many subdivisions in each chapter. The person looking for a 'bird's-eye' view of the large field of metalworking should find this book helpful.

WALTER A. BACKOFEN

Department of Metallurgy,
Massachusetts Institute of Technology

Compounds with Condensed Thiophene Rings. Howard B. Hartough and S. L. Meisel. Part of "The Chemistry of Heterocyclic Compounds" Series, Arnold Weissberger, Ed. Interscience, New York–London, 1954. xv + 515 pp. Illus. Single copy, $16.50; subscription, $15.

The present volume is a companion of *Thiophene and Its Derivatives* and both belong to the Weissberger series, "The Chemistry of Heterocyclic Compounds." The present book is primarily a reference work rather than a critical review of the current literature. Apparently every attempt was made to be exhaustive up to 15 May 1952, the cut-off date.

The 460-odd pages are divided into eight chapters. The first is devoted mainly to an inconclusive discussion in electronic terms of electrophilic substitution in thiophene thianaphthene, dibenzothiophene, and their oxygen, selenium and tellurium isosters. However, this approach is soon dropped, and the remainder of the book is a straightforward exposition of the chemistry of condensed ring systems containing thiophene. Almost half of the book is devoted to thianaphthenes and the closely related thioindigo dyes. The rest is divided among approximately 200 different ring systems. It is not surprising that in many cases only one or two references are cited for a particular ring system and in several instances the authenticity of the formulation is doubtful.

The value of a compendium such as this will depend to a large extent on the excellence of the index. The bibliography, which is located at the back of the book, starts out alphabetically, but after a few hundred references it degenerates into a helter-skelter list of entries. Following this section is a patent bibliography containing more than 700 entries, most of which are accompanied by the *Chemical Abstracts* reference. There is a ring index and a subject index. For some reason there is a supplementary list of patents pertaining exclusively to thioindigo dyes. This list occupies 10 pages and is of no obvious use to anyone except a specialist in this field.

Organic chemists are grateful to the authors for undertaking this monumental task of collecting and collating all the data on thiophene and its derivatives. This volume and its predecessor will be of invaluable assistance to all those interested in thiophene chemistry.

S. ARCHER

Sterling-Winthrop Research Institute,
Rensselaer, New York

The Actinide Elements. Glenn T. Seaborg and Joseph J. Katz, Eds. McGraw-Hill, New York, 1954. 870 pp. Illus. $11.75.

This book, which is composed of sections written by competent workers in the field, summarizes the research on the radioactive elements from actinium to californium. As this volume includes a variety of topics and will be useful primarily for reference, the chapter titles and authors are listed here: "Introduction," Seaborg; "The chemistry of actinium," Hagemann; "Nuclear properties of uranium, protactinium, and thorium isotopes," Katzin; "The chemistry of thorium," Katzin; "The chemistry of protactinium," Elson; "The chemistry of uranium," Hoekstra and Katz; "Nuclear properties of the plutonium isotopes," Seaborg; "Oxidation states, potentials, equilibria, and oxidation-reduction reactions of plutonium," Connick; "Ionic and molecular species of plutonium in solution," Hindman; "Preparation and properties of the compounds of plutonium," Cunningham; "Nuclear properties of the neptunium isotopes," Seaborg; "The chemistry of neptunium," Cunningham and Hindman; "Nuclear properties of the transplutonium nuclides," Seaborg; "The chemistry of the transplutonium elements," Perlman and Street; "Radiochemical separation of the actinide elements," Hyde; "Radiochemical assay by alpha and fission measurements," Jaffey; "Correlation of properties as actinide transition

series," Seaborg; "Crystal chemistry of the 5f elements," Zachariasen; "Optical properties of some compounds of uranium, plutonium, and related elements," Staritzky and Truitt; and "Slow-neutron and spontaneous-fission properties of heavy nuclei," Huizenga, Manning, and Seaborg.

In general, the book is well written; much of it is, however, detailed listings of preparations and properties. Since most of these data are available only in declassified reports and have not heretofore been subject to critical review, this attention to detail is both necessary and desirable. Considerable care must have been taken in the writing, editing, and printing because only four errors were noted. There is a subject index, but unfortunately no author index.

The title of this book, *The Actinide Elements*, presumably stems from Seaborg's theory that these elements form an Actinide Series akin to the well-known Lanthanide Series. Since some chemists do not agree with this concept, Seaborg presents strong arguments in support of his views. An alternative point of view —that this is a Thoride Series—is ably discussed by Zachariasen in his chapter. Other chapters that were found particularly interesting are those by Connick and Hindman on plutonium chemistry.

These authoritative chapters are a fitting "record" of the quality and quantity of the work done on the Plutonium Project. This book is highly recommended to those interested in the chemical and nuclear properties of these heavy elements.

JOHN O. EDWARDS
Metcalf Chemical Laboratories, Brown University

Calculations of Analytical Chemistry. Leicester F. Hamilton and Stephen G. Simpson. McGraw-Hill, New York–London, ed. 5, 1954. xii + 340 pp. Illus. $5.

The fifth edition of this well-known textbook retains the best features of the previous editions. Through selective editing, rewriting, and additions by the authors, its general usefulness to teachers and to students of elementary analytic chemistry has been significantly improved.

For the student, the value of such a problem book, used in conjunction with a standard analytic textbook, lies in the fact that an organized approach to the whole area of basic analytic calculations is available in compact form. Example problems worked out to illustrate the principles of calculations for each type of analytic stoichiometry, together with an ample number of selected problems with and without answers, provide a source of self-instruction for the student and an opportunity to test his knowledge and understanding of the subject.

For the instructor, the value of such a problem book stems directly from the above. The obvious benefit is that less time needs to be spent in organized lecture or recitation presentation of problem work, but more important is the fact that, with adequate printed instructional material available, more efficient use can be made of the time given to individual instruction. During some years of using this textbook, it has been my experience that student questions relating to the material covered are individual questions and differ from student to student and from problem to problem. This opportunity for individual instruction allows for a probing of the student's attempt at self-instruction, and it has been my uniform experience that the student who honestly attempts to understand the material presented in Hamilton and Simpson's book has no trouble in mastering the solution of any problem in basic analytic chemistry.

The following material has been added: a chapter on colorimetry; a brief treatment of precision measures, rejection of results, and the use of nomographs; sections on decomposition potentials, overvoltage, polarization, and electrolytic separations; potassium bromate and iodate titrations; and reference is made to antimony electrodes and examples are given to illustrate the calculation of potentials during the course of a redox titration. Rewritten sections expand the treatment of polarography as related to amperometry, and the Latimer convention regarding electrode potentials is now employed.

Problems relating to these subjects have naturally been added, but many long, seldom-used problems have been eliminated. The total problems are some 40 fewer than in the fourth edition. Editing has corrected most of the errors in the previous editions.

Future editions could include a brief treatment of the statistical theory of the distribution of errors, together with appropriate curves to make the presently defined precision measurements more meaningful. It is hoped that the Fe^{+2}, Ce^{+4} example of a redox titration can be eliminated. Too often the student is led to believe that all equivalence half-cell potentials are the average of the respective E^0 values. Reference is made to problem 16.7 which is so mistakenly solved. The derivation of a universal expression for calculating the potentials and the use of an appropriate example would be desirable in all elementary treatments of the subject.

JOHN M. SCHEMPF
Department of Chemistry,
The Pennsylvania State University

Animal Breeding. Laurence M. Winters. With additional chapters by William Rempel and John N. Cummings. Wiley, New York, and Chapman & Hall, London, ed. 5. 1954. ix + 420 pp. Illus. $5.75.

Modern concepts of livestock breeding are clearly presented in the fifth edition of this textbook. The author draws freely on his own experiences in animal-breeding research, particularly with swine. Approximately half of the book is devoted to selection, inbreeding, and crossbreeding, including a chapter on "building superior germ plasm." Emphasis is placed on the use of inbreeding as a tool to aid selection in forming lines. These lines are then used in a crossing program to obtain hybrid vigor.

For those whose statistics background is limited, the subject of selection is clearly presented without resort to technical statistical terms. Perhaps a brief discussion of gene frequency and how selection changes gene frequency would aid still further in giving the reader a better concept of how selection changes a herd or breed. Dominant, epistatic, and overdominant gene actions as possible explanations of hybrid vigor are clearly discussed. The basic Mendelian principles as related to animal breeding are reviewed briefly as a background for the material on selection and breeding systems. Four chapters are devoted to physiology of reproduction and artificial insemination. A chapter on lethals includes a list of all reported lethals in farm animals.

On the whole, *Animal Breeding* is simply and clearly written and should interest the livestock breeder as well as the student being introduced to this subject.

J. A. WHATLEY, JR.

Animal Husbandry Department,
Oklahoma A & M College

Fresh Water from the Ocean. Cecil B. Ellis. Ronald Press, New York, 1954. xi + 217 pp. Illus. $5.

Fresh Water from the Ocean by Cecil B. Ellis is the first book in which the physical forces and the many techniques involved in the separation of salt ions from sea-water molecules have been systematically examined and analyzed. As with many first works in new fields, the author has had to bring together much scattered information as general background for all readers, even those in closely related technical pursuits. The book is informative, useful, and interesting but also, perhaps necessarily, somewhat superficial as a piece of technical writing.

The book is written in informal style, intended to "attract and enlighten those interested persons who otherwise would shun a highly technical treatment." Ellis explains, from an elementary standpoint, the nature of sea-water impurities, the fundamental physical, chemical, and energy factors involved in separating them from water, and classifies the many possible separation methods into those involving (i) the whole volume, (ii) a surface, and (iii) individual ions or molecules. These general methods are further subdivided into "long-range force systems" (pressure, vibration, heat, refrigeration), "sieve processes" (membranous, osmotic, biological), "distillation" (multiple effect, compression distillation, supercritical, solar, and so forth), and "chemical surface methods" (ion exchange, precipitation and so forth). Following a commendably clear but elementary explanation of the basic physical chemistry involved in each separation method, a description of the process and equipment for a very large-scale plant is presented. An economic appraisal of a 1000-million-gallon-per-day plant (about the amount used by New York City) is then outlined for each technically feasible method.

The work is introduced by a considerable amount of useful background material including sea-water composition, purity requirements for potable and irrigation water supplies, average and minimum U.S. energy costs, and some simple thermodynamics leading to statements of maximum heat engine efficiencies and minimum theoretical work requirements in sea-water separation.

The author concludes that, although compression distillation is the least expensive proved method for plants of almost 1-million-gallon daily capacity, multiple-effect evaporation, supercritical distillation, and freezing might be competitive at a 1000-million-gallon-per-day capacity. None of these systems is believed likely to better the 70 ct/1000 gal price forseeable at this time, however. The author predicts that the electric-membrane method (electrodialysis) when fully developed will be less expensive than any other method heretofore proposed, and that in about 10 years it should be possible to obtain fresh water from the ocean at a total cost of about 30 ct/1000 gal, a price not at all out of reach of many large cities and industries. There is little promise, however, in any method thus far suggested, for producing irrigation water at a cost low enough for practical use.

I question the wisdom of aiming the book at the level of the completely nontechnical reader and, by so doing, failing to develop some of the sound technical ideas and economic factors to a fuller extent. If the author's purpose is to encourage and stimulate thought on the problem, I doubt that the layman will be nearly the source of ideas that the reader of some technical background will be. To describe the precipitation of magnesium and sulfate ions from sea water as a "musical chair game of switching partners" seems quite unnecessary and, perhaps, to be the sort of writing that would discourage the reader who has elementary chemistry training.

Prediction of 70-ct-distillation costs and 30-ct-electric-membrane costs per thousand gallons is indeed bold, for not even the manufacturers and developers of these processes and equipment are so optimistic. I believe that $1 is a more realistic figure for distillation methods, and that the 30-ct figure for electric-membrane separation may ultimately be applicable to a saline ground water of perhaps 5000 ppm solutes rather than to sea water with 7 times this concentration.

The consistent use of kilowatt hours for all energy figures including even heat and chemical reaction enthalpy changes is systematic but somewhat inconvenient for those who are more familiar with Btu's and calories. Inclusion of some heat units along with the electric work units would have aided many users.

I found some of the explanations of electrolysis fundamentals exceptionally clear and of material value, even to the technical reader, in understanding the mechanisms and limitations of methods based on these principles. Of somewhat less clarity is the explanation of multiple-effect and compression distillation, where the role of pressure differences might have been more prominently outlined.

The nontechnical reader, for whom this book is intended, should indeed be interested and enlightened by it, and for that purpose the work is a worthwhile endeavor. The book may also clarify some thoughts for more specialized investigators along certain lines of activity in this general field and indicate to them the nature and magnitude of the problems in competing potential methods for sea-water demineralization.

GEORGE O. G. LOF

Denver, Colorado

Titanium and Titanium Alloys. John L. Everhart. Reinhold, New York, 1954. v + 184 pp. Illus. $3.

This book is published as a "pilot" book, and it contains 184 pages in small-book form, 4½ by 6½ in. It represents a well-selected coverage of the published literature on various phases of the technology of the metal that has appeared in recent technical magazines and in company publications. Everhart summarizes recent literature on forming and fabrication, joining, machining and grinding, and applications, as well as on physical and mechanical properties.

The book should prove useful to those not familiar with developments in titanium but whose diversified interests require a general understanding of the progress being made with this metal. Unfortunately the author lacks working contact with the metal, and he offers only information appearing in the literature, some of which is conflicting and left unresolved.

The many references cited allow easy access to original articles and offer the reader the source of significant information on work accomplished in the various fields covered.

BENJAMIN S. MESICK

Ordnance Corps, Watertown Arsenal, Watertown, Massachusetts

Biological Effects of External X and Gamma Radiation. Part I. Raymond E. Zirkle, Ed. McGraw-Hill, New York–London, 1954. xxvi + 530 pp. Illus. $7.25.

The work reported in this volume was part of a large radiobiological program pursued during World War II at the Metallurgical Laboratory, University of Chicago, and the National Cancer Institute. Although the range of topics is quite wide, all the investigations were primarily directed toward an understanding of radiobiological actions on mammals and on man in particular. This book brings together in one volume many scattered reports that have not been previously available to many. It presents the reports with much detail as to methods and results.

The effects of long-continued total-body irradiation on mice, guinea pigs, and rabbits are reported by Lorenz, Heston, Jacobson, Shimkin, Eschenbrenner, M. K. Deringer and J. Doniger, Schweisthal, Miller, Hagen, and Sacher. Hematological effects are covered more specifically in chapters by Jacobson, Marks, Simmons, Hagen, Zirkle, Sacher, Pearlman, Gaston, Block, Allen, Sanderson, Milham, and Kirshon. Biochemical studies of irradiated animals are reported in chapters by Barron, Muntz, Dickman, Singer, Wolkowitz, Wattenberg, and Schwartz.

The effects of x-rays on immunity are discussed by W. H. and L. G. Taliaferro. There is also a chapter by Simmons, Jacobson, Pearlman, and Prosser on the effectiveness of drugs in preventing or alleviating x-ray damage. The final chapter is on the methods of exposing animals to x-rays by Hagen and Zirkle.

Among other reasons, the book is of value since it represents the type of data on which much of our "acceptable exposure" concepts are based.

TITUS C. EVANS

Radiation Research Laboratory, College of Medicine, State University of Iowa

Advances in Cancer Research. vol. II. Jesse P. Greenstein and Alexander Haddow, Eds. Academic Press, New York, 1954. xi + 530 pp. Illus. $11.

This volume is of considerable interest to the biochemist and to the geneticist, as well as to the physician.

Alexander elaborates on the reaction of carcinogenic substances with macromolecules, and Badger elaborates on the relationship between the chemical constitution of carcinogens and their tumorigenic activity. In my opinion the terms "carcinogen" and "carcinogenic" are often inadequate, and they should be replaced by "cancerogen" and "cancerogenic." The geneticist will find a great deal of information in Law's chapter, and the physician will find the other contributions helpful.

Berenblum sees in cancer pathogenesis a two-stage-mechanism. The first stage (initiation) is sudden and probably due to cell mutation, while the second stage (promotion) is gradual, converting a dormant cancer cell into a tumor. The cancer cell is hidden among the great multitude of nonneoplastic cells of which the precancerous lesion consists.

Brues develops the role of ionizing radiations in cancerogenesis as well as in therapy. Two chapters are devoted to chemotherapy—a comprehensive one by Stock, and a chapter on nitrogen mustards by Klopp and Bateman. In a very informative chapter, Oberling and Guering deal with the virus problem; in another chapter, Fenninger and Mider deal with metabolism in cancer. Craigie covers many details on the survival of tumors in the frozen state.

To each chapter a comprehensive bibliography is appended. The volume is an excellent reference book; it shows the development of experimental cancer knowledge and points to some problems that are to be tackled in the near future.

SIGISMUND PELLER

New York, New York

Technical Papers

Hemoglobin E, a Hereditary Abnormality of Human Hemoglobin

Amoz I. Chernoff and Virginia Minnich

Department of Medicine, Washington University School of Medicine, St. Louis 10, Missouri

Soodsarkorn Chongcharoensuk

Department of Pediatrics, University of Medical Sciences, Siriraj Hospital, Bangkok, Thailand

During the course of experiments (*1*) designed to determine why some Thai patients with Mediterranean anemia (*2*) failed to fulfill the genetic criterions postulated for that disease (*3*), an abnormal hemoglobin was detected that had an electrophoretic pattern different from that of other known hemoglobin types (*4–6*). In accordance with recommended nomenclature (*7*), the new pigment was called hemoglobin E. At about the same time, Itano, Bergren, and Sturgeon (*8*) identified an abnormal hemoglobin, which they designated as hemoglobin E, in a child with an atypical anemia. A sample of blood from one of our subjects was sent to them; the two hemoglobins had a similar electrophoretic mobility and are presumably identical (*9*).

We have now found hemoglobin E in the blood of eight Thai subjects. Five of these eight subjects had clinical and hematologic manifestations of Mediterranean anemia; electrophoretic analysis revealed hemoglobin E and hemoglobin F. In four instances it was possible to show that one parent had the Mediterranean trait. The disease may properly, therefore, be called Mediterrean-hemoglobin E disease (*4, 7*). The remaining three subjects were asymptomatic; no specific hematologic abnormalities were recognized. Their blood contained hemoglobins E and A. They are regarded as having the hemoglobin-E trait.

Hemoglobin E was identified by a slight modification of the paper electrophoresis technique described by Smith and Conley (*5*). Electrophoretic runs were made at room temperature for 10 to 14 hr at 350 v and 14 to 16 ma. A sheet of Whatman 3 MM paper, 7.5 in. wide, was placed between siliconized glass plates 8 by 14 by $\frac{3}{8}$ in. which were secured together by heavy clamps to prevent puddling of the veronal buffer (*p*H 8.8 and ionic strength 0.06). The paper was marked with a vertical line through the center, soaked in buffer, and blotted almost dry. With a micropipette, 0.003 to 0.005 ml of a 6 to 12 gm percent hemoglobin solution (*10*) was placed on the paper at the center line. Ten specimens could be run simultaneously. The spots could be followed without difficulty as they migrated, and they were clearly visible without staining after the paper had been dried in an oven at 100°C for 30 min. The percentage of fetal hemoglobin was determined by the alkali denaturation technique (*10, 11*).

When hemoglobin solutions are subjected to paper electrophoresis under the conditions outlined, hemoglobin A moves farthest toward the anode in the electric field of the paper, while hemoglobin F moves slightly more slowly and is imperfectly separated from hemoglobin A (*5, 6*). Hemoglobin S migrates still more slowly, and hemoglobin C migrates least of all. The new hemoglobin identified in the Thai subjects moved with a mobility intermediate between that of hemoglobins S and C so that the spot at the end of the run was between those of hemoglobins S and C (Fig. 1). A small quantity of hemoglobin E was prepared relatively free of other types of hemoglobin by elution of the hemoglobin-E spots from filter paper strips. The eluate was dialyzed against four changes of distilled water and analyzed spectrophotometrically in a Beckman spectrophotometer. The absorption spectrum was characteristic of oxyhemoglobin; no deviations from the curve of normal adult hemoglobin in the visible range were detected.

The genetic transmission of hemoglobin E is indicated by a study of the following family in which the father and his three children had hemoglobin E in their blood (Table 1). The mother fulfilled the criterions for the Mediterranean trait: hypochromic polycythemia, a marked decrease in osmotic fragility, and numerous target cells. Electrophoretic study of her hemoglobin showed hemoglobin A, while, by the alkali denaturation test, hemoglobin F was found in normal amounts (*11*). The father was asymptomatic, and he showed minimal hypochromia, a slight decrease in

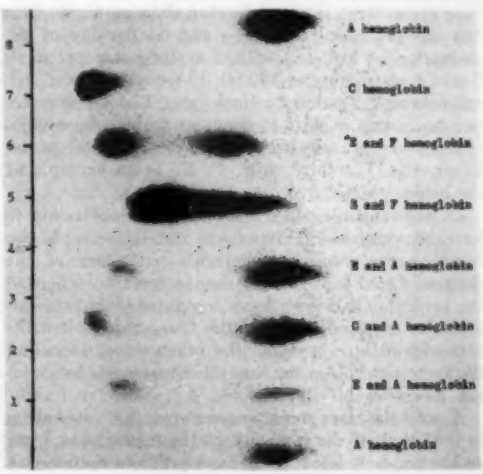

Fig. 1. Paper electrophoresis of a number of hemoglobin specimens containing various types of hemoglobin. Numbers 2 and 4 are examples of hemoglobin-E traits, and No. 6 is an example of Mediterranean-hemoglobin E disease.

Table 1. Hemoglobin components in members of Thai family "K."

Subject	Sex	Age (yr)	Hemoglobin types*			Percentage of hemoglobin F†	Remarks
			E	A	F		
Mrs. K.	F		–	–	–	Normal	Mediterranean trait
Mr. K.	M		+	–	–	Normal	Hemoglobin-E trait
D. K.	F	2½	+	–	+	13.6	Mediterranean-hemoglobin E disease (transfused)
C. K.	M	1	+	–	+	33.0	Mediterranean-hemoglobin E disease
P. K.	F	7	+	+	–	Normal	Hemoglobin-E trait

* By paper electrophoresis method.
† By alkali denaturation technique.

osmotic fragility, and only a few target cells. Electrophoretic analysis of his hemoglobin revealed hemoglobin E in association with hemoglobin A; he was regarded, therefore, as having the hemoglobin-E trait. Two children (D. K. and C. K.) were found to have Mediterranean-hemoglobin E disease, a severe hemolytic anemia, indistinguishable from Mediterranean anemia but with both the E and F types of hemoglobin present. The small amount of hemoglobin A found in D. K. almost certainly resulted from a recent transfusion. Some of the characteristics of the disease are evident in the following data on C. K. This boy has hepatosplenomegaly, and on the day of examination he had 2.24 million erythrocytes per mm^3, 5 g of hemoglobin per 100 ml, 19 percent packed red-cell volume, 8.2 percent reticulocytes, 17,450 leucocytes per mm^3 with a shift to younger forms of granulocytes, and 40 nucleated erythrocytes per 100 white-blood cells. The third child, P. K., is an example of the hemoglobin-E trait.

Of the remaining four subjects who were found to have hemoglobin E, three were unrelated, while the fourth subject was the maternal half-sister of the children listed in Table 1; she presumably inherited the hemoglobin-E gene from her father who was not available for study. One of the four subjects had the hemoglobin-E trait, while the other three, including the maternal half-sister, had Mediterranean-hemoglobin E disease.

Among the cases so far encountered, it is interesting to note that, if the intensity of the hemoglobin spots on the paper is taken as a rough guide to the amount present, hemoglobin E forms the major component in Mediterranean-hemoglobin E disease and the minor component in the hemoglobin-E trait.

Two additional families in which Mediterranean anemia has occurred have been examined; one of these was included in the report by Minnich *et al.* (*2*) in their study of Mediterranean anemia in Thailand. Only hemoglobins A and F were found, the expected pattern in Cooley's anemia. Further studies of the occurrence and significance of hemoglobin E are currently in progress in Thailand.

References and Notes

1. This work was supported by Grant No. H-22 C(7) from the National Heart Council of the U.S. Public Health Service and by the Mark A. Edison Memorial Fund. The authors wish to acknowledge their appreciation for help given by Donald Ruchnagel and Su-ed Kochaseni.
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20 April 1954.

Experimental Production of Renal Glycosuria, Phosphaturia, and Aminoaciduria by Injection of Maleic Acid*

Harold E. Harrison and Helen C. Harrison

Department of Pediatrics, Johns Hopkins University School of Medicine and Pediatric Division, Baltimore City Hospitals

Berliner, Kennedy, and Hilton (*1*) reported that the intravenous injection of maleate into acidotic dogs interfered with the renal tubular mechanisms necessary for excretion of acid urine. Impairment of other renal tubular functions also resulted, and the urinary excretion of phosphate was increased. In a footnote the authors commented on a possible reduction of phosphate Tm. We have suggested that one of the effects of vitamin D is to increase renal tubular reabsorption of phosphate (*2*) and became interested in maleic acid as a possible inhibitor of a renal tubule system that is influenced by vitamin D.

When maleic acid, neutralized to *p*H 7.0 with NaOH, was injected intraperitoneally as a 0.1*M* solution into rats fed a low phosphate diet, an increased phosphate loss in the urine occurred. In the following experiments, 6-wk-old rats that had been maintained on a high-calcium, low-phosphorus, rachitogenic ration for 3 wk were placed in metabolism cages permitting quantitative collection of urine uncontaminated with feces or diet. The following substances were determined in the urine by the methods listed: phosphorus, Fiske and Subbarow (*3*); calcium, Kramer and Tisdall (*4*); citrate, Natelson *et al.* (*5*); and

Table 1. Effect of intraperitoneal injection of maleate upon excretion of calcium, citrate, phosphorus, and amino acids by rachitic rat; No. 85A, wt. 112 g.

Day	Maleate (ml of 0.1*M* soln.)	Urine excretion			
		Ca	Citrate (mg/24 hr)	P	Amino acids (μM/24 hr)
1	0	18.2	18.7	0.05	62
2	0	16.6	18.2	.04	65
3	1.0	6.5	11.8	1.53	481
4	1.0	6.9	8.8	0.63	382
5	2.0	5.3	8.5	.45	147

amino acids by an adaptation to urine of the ninhydrin spectrophotometric technique of Troll and Cannan (*6*).

Table 1 shows the results of a typical experiment in which the given dose of maleate was injected intraperitoneally once daily starting on the third day. In the control period only traces of phosphorus were excreted in the urine, which is to be expected of rats fed this low phosphate diet. Following maleate there was a marked increase in urine phosphorus and concomitantly there was a manyfold increase in urinary amino-acid excretion. The urinary excretion of calcium and citrate, on the other hand, were both diminished. Control rats injected with succinate or malate, in equimolar amounts, showed no increase in urinary phosphate or amino-acid excretion and no decrease in urine calcium or citrate excretion.

Resistance to the maleate effect develops rapidly. In the experiment described, the phosphaturia and aminoaciduria had decreased by the third day of maleate treatment, even though the dose was increased. In several of the rats treatment was discontinued at this point and then resumed after a 5- to 7-day interval. The maleate effect could be elicited again with redevelopment of resistance after several days of the second course of treatment. No evidence of permanent renal injury was found in rats receiving 1.0 ml of 0.1*M* maleate per 100 gm body weight daily for periods of 2 to 3 wk.

Table 2. Glycosuria, aminoaciduria, and phosphaturia following intraperitoneal injection of maleate; rat No. 81A, wt. 70 g.

Day	Maleate (ml of 0.1*M* soln.)	Urine excretion				
		Ca	Citrate (mg/24 hr)	P	Glucose	Amino acids (μM/24 hr)
1	0	23.0	25.4	0.02		80
2	0	19.1	20.0	.03		63
3	0	18.5	20.4	.05	< 6	69
4	1.0	9.4	16.5	1.54	168	566
5	2.0	10.0	16.5	0.93	116*	344
6	2.0	12.2	12.1	.13	38	177

* Blood sugar on this day, 80 mg/100 ml.

Tests of the urine with Benedict's reagent revealed the presence of a reducing substance in the urine of rats injected with maleate, and this was identified as glucose. The concentration of glucose in urine and blood was determined quantitatively by the Somogyi-Nelson method (*7*). Table 2 gives the results of one such study showing the glycosuria appearing simultaneously with the increased urinary excretion of phosphate and amino acids.

It was thought that the increased urinary losses of glucose, phosphate, and amino acids were most likely caused by impairment of renal tubular reabsorption of these solutes and it could readily be shown that the plasma levels of glucose or phosphate were not elevated by maleate injection. In two rats urine was collected for 6 hr following a single injection of maleate, and blood samples were obtained at the end of this period. The blood-glucose levels were 102 and 104 mg/100 ml, and the urinary excretions of glucose were 34 and 3.7 mg, respectively, over the 6-hr period. The serum-phosphorus concentrations were 1.9 and 1.5 mg/100 ml, and the urine contained 0.19 and 0.08 mg of phosphorus. Blood-glucose levels were also determined in four rats on the second day of maleate treatment. The animals were not fasted. The blood-sugar concentrations ranged from 77 to 90 mg/100 ml. In three of the maleate-treated animals the serum-phosphorus levels were found to be 2.0, 2.1, and 2.4 mg/100 ml in comparison with the average value of 2.9 mg/100 ml for seven control rats on the same diet.

Plasma amino acids were not determined. Identification of the urine amino acids by paper chromatography revealed that the excretion of leucine, iso-leucine, valine, methionine, histidine, alanine, and glutamic acid was increased following maleate. Since amino acids of the leucine, iso-leucine, valine group have been shown to be reabsorbed almost completely by the renal tubules in the normal dog even at elevated plasma levels (*8*), their excretion in increased amounts also suggests interference with renal tubule mechanisms.

In a total of 18 rats thus far studied, the maleate effect described here has been consistently elicited. In three rats 20,000 units of vitamin D were given 3 days prior to maleate injection. The increased loss of phosphate, glucose, and amino acids in the urine was not prevented by this large does of vitamin D.

The triad of renal glycosuria, phosphaturia, and aminoaciduria produced by maleate is characteristic of a congenital metabolic defect in man known as the Fanconi syndrome (*9, 10*). In this inborn error of metabolism, rickets or osteomalacia resistant to ordinary treatment with vitamin D is also found. If the locus of maleate effect on renal tubular functions can be determined, it may be possible to characterize the defective enzymatic mechanisms in the Fanconi syndrome and determine whether a single defect or multiple defects are present. Investigation of the basis for resistance to vitamin D in subjects with impairment

of renal tubular function will help in understanding the physiology of vitamin D. The experiments reported suggest an interrelationship of the metabolism of the polycarboxylic acids and specific renal tubular functions.

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* Aided by a grant from the Playtex Park Research Institute.

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1 June 1954.

Influence of Hydrogen Ion Concentration on Radiation Effects

Tikvah Alper and M. Ebert

Experimental Radiopathology Research Unit, Medical Research Council, Hammersmith Hospital, London

When ionizing radiation acts on chemical or biological systems by "indirect effect," that is, through the medium of free radicals, the changes are very often oxidations or reductions. It is commonly supposed that the majority of radiobiological changes are oxidative, because the presence of oxygen during irradiation enhances the effects (*1, 2*). Evidence was presented earlier by one of us (*3–5*) that the inactivation of phages S13 and T3 by indirect action of radiation is due to reduction, so that oxygen in the suspension protects the phage, whereas removing oxygen and bubbling hydrogen during irradiation enhances "immediate" radiation effects (*6*). More recently, Bachofer and Pottinger (*7*) found a protective effect of oxygen on T1, and it may be that all phages are sensitive, in the free state, to inactivation by reducing agents.

We have shown (*6*) that, under electron or x-irradiation, the mechanisms of hydrogen peroxide formation in water and immediate phage inactivation in aqueous suspensions can to some extent be regarded as complementary. Under a wide variety of gas treatments, the inactivation of phage proceeded fastest when the hydrogen peroxide formed was least, and vice versa. We showed that the likely reactions leading to formation of hydrogen peroxide and phage inactivation could be fitted into a simple theory of radical formation and reaction. This theory would lead to a dependence on hydrogen ion concentration of the yield for both oxidative and reductive changes, and we are now able to show that in this respect, too, phage inactivation and hydrogen peroxide formation proceed in complementary fashion.

The *p*H dependence of these reactions arises from the step

$$HO_2 \rightleftarrows H^+ + O_2^-,$$

reductive changes being due to the radical ion O_2^-, so that oxidation yields are increased, and reduction yields decreased, in more acid solutions. However, the extent of reaction with O_2^- will depend on *p*H only in the presence of oxygen, since the formation of the HO_2 radical is a preliminary step. As is shown by Fig. 1a, phage is protected against inactivation in acid

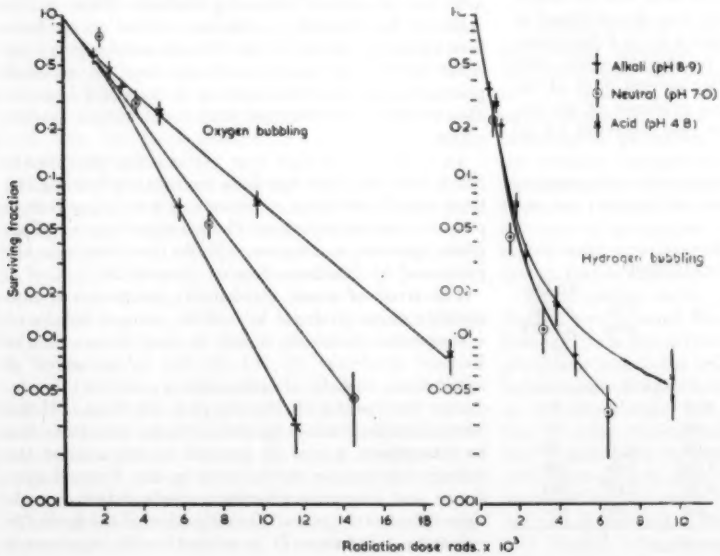

Fig. 1. Survival curves of bacteriophage S13, x-irradiated under (*a*, left) oxygen, (*b*, right) hydrogen bubbling, at three hydrogen ion concentrations.

suspension under oxygen bubbling, whereas in alkaline suspension radiation effects are enhanced. Under hydrogen bubbling there is much less *p*H dependence (Fig. 1b). These facts would appear to support the hypothesis that O_2^- radical ions are inactivating agents for phage. The *p*H dependence under hydrogen bubbling may arise from the reaction proposed by Weiss (*8*),

$$H + H^+ \rightleftarrows H_2^+.$$

The effects of *p*H variation on formation and decomposition of H_2O_2 were reported previously by Ebert and Boag (*9*). As Fig. 2 shows, the equilibrium

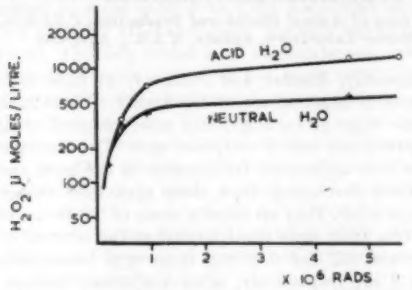

Fig. 2. Formation of H_2O_2 by electron irradiation under oxygen bubbling, at two hydrogen ion concentrations.

yield of H_2O_2 under oxygen bubbling was higher in more acid solution, indicating that the back reaction (decomposition of H_2O_2) became equal to the forward reaction (formation of H_2O_2) earlier, in more alkaline solution. The formation of H_2O_2 under nitrogen bubbling could not be followed by the methods used, because the yields were not measurable. The effect of varying the *p*H was therefore studied by means of decomposition reactions, and, as is shown by Fig. 3, these were very much slower in more acid solution.

Although the dependence of radiation effects on *p*H is well established in radiation chemistry and is to be predicted from theories of radical reactions (*10*), only a few biological systems appear to have been studied

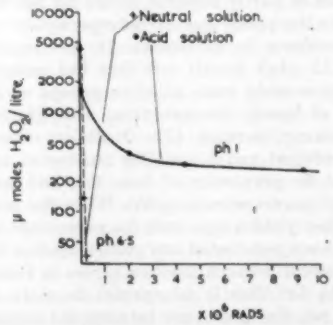

Fig. 3. Decomposition of H_2O_2 by electron irradiation under nitrogen bubbling, at two hydrogen ion concentrations.

from this point of view (*11–13*). The nonuniform distribution, within the cell, of metabolites, enzymes, and colloidal matter probably involves local and temporal variations in *p*H and in the concentration of dissolved substances. If radiation-produced radicals are responsible for some effects in the living cell, then a complicated pattern of radiosensitivity is to be expected on these grounds alone.

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7 June 1954.

Theoretical Rate of Fat Formation by Yeasts

M. P. Steinberg and Z. John Ordal

Department of Food Technology, University of Illinois, Urbana

Synthesis of fat by microorganisms has interested a number of investigators since the early work of Lindner (*1*). We recently reported on some studies concerning the effect of several cultural conditions on fat formation by *Rhodotorula gracilis* (*2*). In this work the fat content of the yeast was expressed as a weight ratio of fat to nonfat dry yeast. When this fat ratio was plotted against time, the curve was a straight line after an initial lag period. This indicated that the cells produced an equal quantity of fat during like intervals of time. Thus the fat was produced at a constant rate which was represented by the slope of the linear portion of the curve. We have termed this slope the *fat rate*; it is expressed in grams of fat per 100 g nonfat dry yeast per hour. It was of interest to compare experimental fat rates with the maximum rate theoretically possible. Such a comparison would indicate whether a larger rate of fattening could be obtained as a result of further investigations on this problem.

The relationship between the energy required for growth of the yeast and that required for synthesis of fat was the basis used in deriving a theoretical fat rate. Thus, a given rate of growth is evidence of a definite rate of energy utilization. If this energy were applied solely to the formation of fat, then a corresponding rate of fattening would be obtained. This theoretical fat rate was derived as follows.

The weight of cellular material present in the logarithmic growth phase of a microbial fermentation may be expressed as

$$W = W_0\, 2^{(t/g)},$$

where: W is the weight of cells present at time t, W_0 is the weight of cells present at time $t = 0$, and g is the generation time. Converting to natural logarithms and differentiating,

$$\ln W - \ln W_0 = (t/g)\ \ln 2,$$

$$\frac{dW/dt}{W} = \frac{0.693}{g}.$$

Thus, the instantaneous rate of cell increase per unit weight of cells present is inversely proportional to the generation time. Taking the observed generation time for *Rh. gracilis*, namely, $g = 2.8$ hr, we find that the rate of dry weight increase in grams per hour per 100 g of dry yeast present is $100 \times 0.693/2.8 = 25$. The cellular material being synthesized at this rate is composed of carbohydrate and protein; that is, almost no fat is being produced.

If we assume that, on a weight basis, the energy content of fat is 2.5 times that of carbohydrate or protein (*3*), we find the maximum rate of fat production to be

$$\frac{25\ \text{g carb.} + \text{prot.}}{100\ \text{g yeast/hr}} \times \frac{1\ \text{cal/g carb.} + \text{prot.}}{2.5\ \text{cal/g fat}}$$

$$= 10\ \text{g fat per 100 g nonfat dry yeast per hour.}$$

Therefore, assuming that the energy transport enzyme system of the cell is as efficient during fat production as when carbohydrate and protein are being formed, we may conclude that the maximum rate of fat formation by *Rh. gracilis* would be 10. Calculations on other microorganisms, for example, *Torulopsis utilis*, showing the same generation time, should give the same results.

The converse of this conclusion must also be examined. If the rate of fattening is below the theoretical value calculated in preceding paragraphs, then we may state that the slowest or rate-determining steps occur in the fat-forming process and not in the respiratory or energy-yielding system leading up to it.

We may now compare this theoretical maximum fat rate with results obtained under experimental conditions. Enebo *et al.* (*4*) presented data on the fattening of *Rh. gracilis* at two levels of inoculum under otherwise similar conditions. These data were recalculated in order to determine fat rates. At both levels of inoculum, the calculations showed the fat rate to be 2.0. Kleinzeller (*5*) reported that *T. lipofera* increased in fat content from 23 to 33 percent in 9 hr. If we assume that these data came from the linear portion of the fattening curve, the calculated fat rate would be 2.2. The maximum fat rate found in our work, using *Rh. gracilis*, was 3.1. It is to be noted that these several experimental fat rates are about one-fourth of that theoretically possible as derived in preceding paragraphs. This suggests that further investigations should reveal the conditions required for more rapid synthesis of fat by microorganisms.

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5 May 1954.

The Fertile Life of Mouse and Rat Eggs

A. W. H. Braden and C. R. Austin

Division of Animal Health and Production, C.S.I.R.O., McMaster Laboratory, Sydney, N.S.W., Australia

Recently, Runner and Palm (*1, 2*) have reported ingenious experiments on the transfer of unfertilized mouse eggs at various times after induced ovulation to previously mated recipient mice. The transplanted eggs thus underwent fertilization in the host, and the foetuses developing from these eggs were recognized by eye color. They recorded a mean of 1.1 living young at term from eggs transplanted at the time of ovulation and 0.2 and 0 young from eggs transplanted 4 and 8 hr, respectively, after ovulation. Runner and Palm interpreted their results to mean that a delay of 4 hr or more between the ovulation and the fertilization of mouse eggs would cause a large reduction in the number of eggs capable of normal development. They remarked that their findings were consistent with those previously reported for other species in indicating that fertilization must commence during a "precariously short" interval after ovulation if mammalian eggs are to develop normally to term.

Some of the observations that we have made also bear upon the problem of the length of the fertile life of mouse and rat eggs. Our findings, while confirming the reduction in fecundity after delayed fertilization, indicate that most of the eggs retain their capacity for normal development for a much longer period than that implied by Runner and Palm. It also appears that the losses observed after delayed fertilization can be partly imputed, in the rat but not in the mouse, to the occurrence of polyspermy.

Our evidence is as follows. In one experimental series, 115 adult female rats that had mated during the early evening were killed in groups of 10 to 30 animals at hourly intervals from midnight to 7 A.M. the following morning (*3*). Ovulation occurred between midnight and 4 A.M. The number of eggs ovulated and the proportion of these that had been penetrated by sperms were recorded. When the percentage of rats that yielded eggs and the percentage in which all eggs were penetrated are plotted against time, the mean interval between the two curves is found to be about 5½ hr. This is interpreted to mean that, in any one rat, the mean time between the beginning of ovulation and the penetration of all the eggs by sperms was about 5½ hr.

The procedure was repeated with 100 rats that had

been kept under conditions of controlled illumination, the times of light and dark being such that ovulation took place between 11 A.M. and 3 P.M. (*3*). The average interval, in any one rat, between ovulation and the penetration of all the eggs was estimated in the same manner and again was found to be about 5½ hr.

Similar observations, as yet unpublished, on 150 mice kept under controlled lighting conditions showed that, for any one mouse, the average interval between ovulation and the penetration of the majority of the eggs was 6 to 7 hr.

In a fourth experiment, 14 oestrous rats that had been kept under normal colony conditions were not permitted to mate until 9 to 9:15 A.M.—that is, between 5 and 9 hr after ovulation. We have shown elsewhere (*3, 4*), that, under such circumstances, sperm penetration into the eggs does not begin until 2 to 4 hr after mating and then reaches completion, in any one rat, in 1 to 2 hr. The eggs of these rats must, therefore, have been penetrated by sperms between 8 and 15 hr after ovulation. The rats were allowed to carry their litters to term; a total of 91 apparently normal young (45 ♂♂ and 46 ♀♀) were born and later weaned, corresponding to a mean litter size of 6.5. The mean litter size at weaning of normally mated rats in this colony is 7.7.

Blandau and Jordan (*5*) obtained comparable results from rats artificially inseminated about 6 hr after ovulation; they recorded a mean litter size of 4.6 as compared with 6.7 when insemination was effected before ovulation. When allowance is made for the interval of 3 to 6 hr, which, as already mentioned, occurs in rats between late mating and the penetration of all the eggs, it is seen that the actual time of sperm penetration in their experiments must have been 9 to 12 hr after ovulation.

Mating between 8 and 10 A.M. on the morning of ovulation has been found to result in dispermy in about 9 percent of fertilized rat eggs, and this condition almost certainly leads to triploidy in the embryo (*6*). Chromosome counts on testis squash preparations from 40 to 45 male rats born in the fourth experiment just described revealed no instance of triploidy. This supports the conclusion of Beatty (*7*) that triploid embryos seldom survive to term and partly accounts for the decreased litter size observed in rats after delayed mating.

In short, we have found that, when mating was unrestricted, sperm penetration of all eggs in any one rat was not completed until nearly 6 hr after ovulation, and even when penetration was experimentally delayed until 8 to 15 hr after ovulation, the prenatal loss attributable to the delay was less than 16 percent. The fertile life of most of the eggs in our rats, therefore, was at least 10 hr. The same may have been true also for the mouse eggs, but this was not tested; however, it was shown that in mice allowed free mating, as under colony conditions, the mean interval between ovulation and the penetration of most eggs in any one mouse was as long as 6 to 7 hr. Now, Runner and Palm (*2*) found that when the results of transferring eggs 4 hr after ovulation were compared with those of transferring eggs at the time of ovulation, there was a loss of more than 80 percent of the young. This is in strong contrast to prenatal losses from all causes of 23 percent (unpublished data) and 30 percent (*8*) observed in apparently normal mouse colonies. The much larger loss observed by Runner and Palm must have been in some way associated with manipulation of the eggs *in vitro*, and we conclude that the resistance of eggs to the injurious effects of conditions *in vitro* must diminish rapidly with age.

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10 May 1954.

Nonidentity between Pepsitensin and Hypertensin Revealed by Paper Electrophoresis

A. C. M. Paiva, Therezinha Bandiera, J. L. Prado

Laboratórios de Farmacologia e Bioquímica, Escola Paulista de Medicina, São Paulo, Brazil

Croxatto and Croxatto (*1*) reported the production of a new vasoconstrictor and hypertensive substance when plasma globulins were incubated with pepsin at 38°C and *p*H 2 to 6. This substance, which was named pepsitensin, was subsequently studied by several authors and found to present chemical and pharmacological properties very similar to those of hypertensin (*2*). The only difference between the two substances was found by Braun-Menendez and coworkers (*3*), who showed that crude preparations of pepsitensin are not destroyed by hypertensinase from dog's red blood cells (RCH). However, this was shown to be due probably to the presence in crude pepsitensin of hypertensinase-inhibiting substances; in fact, Vasquez (*4*) succeeded in inactivating as much as 50 percent of a purified pepsitensin by incubating it with a dose of RCH sufficient to destroy completely an equipressor amount of hypertensin. Subsequently, we (*5*) were able to purify further pepsitensin and *completely* abolish its pharmacological activity by a dose of RCH that destroyed hypertensin as well. The similarity between both substances was further strengthened by our work when we found that a mixture of both could not be resolved in its components by paper chromatography with three different pairs of solvents (butanol : acetic acid : water, 4 : 1 : 5; acetone : acetic acid : water, 5 : 1 : 4; dioxane : butanol : water 10 : 10 : 5).

We now report the results of the comparative study

of pepsitensin and hypertensin by means of paper electrophoresis (*6*), using a slight modification of Durrum's technique (*7*). Three strips of No. 1 Whatman filter paper (2.5 by 40 cm) were suspended by their central parts on a horizontal supporting glass rod and their ends were dipped into positive and negative electrode vessels containing a glycine/HCl buffer solution of *p*H 2.5 and 0.1 ionic strength.

After the strips were soaked with the buffer solution, a potential difference of 3500 (*8*) was established between the two poles and the current was maintained for 30 to 60 min and then interrupted. Ten microliters of equivalent solutions (130 Indianapolis units/ml) of pepsitensin and hypertensin was then placed on the center position of strips 1 and 2, respectively, and a mixture containing 5 μlit of each solution was applied to strip 3. The whole system was placed in a chamber where air was completely replaced by nitrogen. The electric current was maintained for 5 to 6 hr, the voltage being kept constant through a voltage regulator; the current usually rose from 6 ma at the beginning of the experiment to 8 ma at the end. The paper strips were then dried at room temperature and cut into pieces 1 cm long that were assayed on the isolated guinea-pig ileum.

The response of this preparation has been shown to be a very sensitive method for the detection of hypertensin (*9*) and pepsitensin (*10*). As Fig. 1 shows, both substances migrated toward the cathode at *p*H 2.5; but pepsitensin had a greater mobility than hypertensin, and they were separated when a mixture of the two was submitted to electrophoresis. Thus, hypertensin and pepsitensin seem to be such similar peptides that their mixture cannot be resolved by paper chromatography with three pairs of solvents, but it can be resolved through paper electrophoresis.

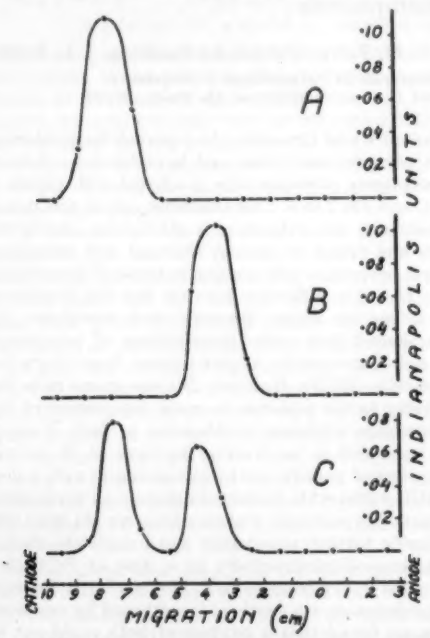

Fig. 1. Migration of pepsitensin, hypertensin, and a mixture of both on paper electrophoresis: *A*, pepsitensin; *B*, hypertensin; *C*, pepsitensin plus hypertensin. Potential difference, 350 v; time, 6 hr; buffer glycine/HCl, *p*H 2.5, ionic strength 0.1. The substances were localized by testing segments of paper on the isolated guinea-pig ileum.

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24 May 1954.

A Method of Assessing Experimental Pulmonary Edema

Milton H. Joffe

Field Toxicology Branch, Cml C Medical Laboratories, Army Chemical Center, Maryland

A technique for assessing pulmonary edema has been successfully used at Medical Laboratories in recent months. The original technique was first shown to me some years ago by Hermann Rahn (*1*) of the University of Rochester, whose interest in respiratory physiology has led him, among numerous other activities, to preserve the lungs of a great number of species of animals by drying them on a compressed-air line. Lung, when minus tissue water and blood and inflated with air, becomes parchment-like yet preserves its morphological features and remains intact for many months with no other treatment. After some years of using this technique to make preparations, it has been adapted to the study of the dynamics of edema formation.

In order to measure the rate of development and the amount of pulmonary edema produced by chemical or physical agents in experimental animals, one must know the weight of the nonedematous lungs. This value is usually calculated from average figures taken from a large sample of untreated animals. Since the weight of the lung as percentage of body weight is quite variable among individuals of all species, obtaining an average figure means the use of considerable numbers of control and experimental animals in order to obtain a reliable figure. We are describing

Table 1. Relationship of lung weights to time after exposure.

Treated.					Untreated			
Body wt. (g)	Lung wt. (g)		Difference (g)	Exposure time (hr)	Body wt. (g)	Lung wt. (g)		Difference (g)
	Wet	Dry				Wet	Dry	
183	1.7	0.32	1.38	1	510	4.3	0.9	3.4
345	3.6	.59	3.01	1	280	2.3	.5	1.8
335	3.7	.605	3.09	1	285	2.7	.5	2.2
670	4.4	.95	3.45	1	730	6.0	1.4	4.6
720	6.8	1.5	5.3	1	332	2.2	0.4	1.8
580	5.15	1.15	4.0	1	550	5.0	1.1	3.9
			3.37	Mean	680	5.4	1.2	4.2
220	3.0	0.44	2.56	2	760	5.7	1.1	4.6
317	5.3	.70	4.6	2	660	7.0	1.4	5.6
279	5.4	.635	4.76	2	440	3.7	0.7	3.0
309	3.6	.535	3.06	2	550	5.1	1.1	4.0
400	5.2	.71	4.49	2	700	4.8	1.1	3.7
387	4.6	1.06	3.54	2	420	2.9	0.6	2.3
670	8.0	1.11	6.89	2	480	3.3	.7	2.6
			4.27	Mean	382	2.7	.6	2.1
450	4.3	0.93	3.37	4	290	2.6	.45	2.2
375	8.4	.87	7.53	4			Mean	3.25
468	6.4	1.56	4.84	4				
620	13.0	1.3	11.7	4				
510	8.3	1.155	7.14	4				
630	12.8	1.66	11.14	4				
			7.62	Mean				

here a method whereby each animal provides its own control value, thus enabling one to consider events in each individual and to reduce greatly the number of animals necessary per experiment.

The physical manipulations involved are extremely simple. The thoracic organs (including trachea as far craniad as the thyroid cartilage) are removed, and the lungs are separated from heart, great vessels, and lymph nodes. Care is taken not to rupture any portion of the bronchial tree. The lungs are then blotted on toweling or filter paper to remove any adhering blood; no attempt is made to drain completely the smaller pulmonary and bronchial vessels at this time. The weight is obtained. A cannula is inserted into the trachea and securely tied. The cannula is then attached to a source of compressed air (preferably in series with a cotton or glass wool filter) and inflated to a firm consistency. This degree of inflation often brings the lungs to somewhat greater than life size. As blood and edema fluid are forced out of the tissue and vessels by expansion of the organ, they are gently removed with gauze sponges, and the normal preparation then has a uniform pink color. The lungs are left inflated for 12 to 24 hr, depending on the gross size, at which time they are firm, smooth, and dry. When completely dried, the cannula is removed and the lungs are weighed again.

A series of guinea pigs treated percutaneously with 20 mg/kg of an aqueous 70-percent solution of phosgene oxime (*2*) and sacrificed at time intervals of 1, 2, and 4 hr provided the data in Table 1. The regression line of weight difference of dose is estimated by $Y = 1.66 + 1.46X$, where Y is the estimated lung weight difference (g) of a guinea pig surviving exposure to agent, and X is the period of exposure (hr). The slope of this regression line is 1.46; that is, about 1.5 g lung weight change per hour exposure is significantly different from zero ($p = 0.05$). The standard error of the slope b is $S.E._b = \pm 0.4$.

References and Notes

1. Rahn's method of preparation of many lungs of many species are well known to those familiar with department of physiology at Rochester. It has so far not been in print.
2. W. Prandtl and K. Sennewald, *Ber.* **62B**, 1754 (1929).

4 June 1954.

Every honest researcher I know admits he's just a professional amateur. He's doing whatever he's doing for the first time. That makes him an amateur. He has sense enough to know that he's going to have a lot of trouble, so that makes him a professional.—CHARLES F. KETTERING.

Communications

Note on a Displaced Dogfish

Squalus acanthias is commonly referred to as being an inhabitant of the waters off the coast of New England and northern Europe. Nowhere can I find mention of its having any proclivity to inhabit fresh water. I would therefore like to report the following incident that occurred 30 August.

Morgan and Nick Garrett (ages 10 and 7, respectively) landed a 27-in. female *S. acanthias* while fishing from a bridge over the Appomattox River just north of Farmville, Virginia, in Prince Edward County. The shark appeared to be in excellent physical condition, making it necessary that the boys club it severely after having pulled it out of the water.

The Appomattox River is an uncommonly sluggish and muddy stream located in the James River drainage area. The point at which the fish was taken is 120 airline miles from the Atlantic Ocean.

C. W. Hart, Jr.
Biology Department, Washington College, Chestertown, Maryland

2 September 1954.

Word Saving, Good and Bad

Herman R. Struck [*Science* **119**, 522 (1954)] advocates methods for getting rid of word padding that are broadly similar to some that I have often used in quasi-editorial work. I think, however, that alternative methods of applying one or two of them are worth considering; that Struck is too hard on *there are*, and so forth, and on the use of passives; that words can be saved in ways that he has not mentioned; and that if word-saving is made an end in itself some other important objectives may be overlooked.

Most of the specific advice in Struck's paper may be summarized in these three precepts: (i) Use strong verbs whenever possible rather than weak verbs coupled with abstract nouns. (ii) Be wary of the inverted forms that begin with *it is, there are,* and so forth. (iii) Whenever possible, use the active, rather than the passive, voice.

Precept (i), as Struck develops it, makes *to be* and other weak verbs "chronic offenders." To me they seem inoffensive except when they get into the bad company of abstract nouns. The most effective way to carry out this precept, I believe, is to look for the abstract nouns —which are easier to find than the weak verbs—and get rid of most of them, chiefly by utilizing the strong verbs from which many of them are derived. "The rock shows much alteration" is a word longer and is weaker than "The rock is much altered." Since many needless abstract nouns end in *-tion,* much of the ground is covered by what used to be a pet slogan of mine: "Shun *-tion*!" Abstract nouns do not all end in *-tion,* however, and we have to use some of those that do end that way as well as many of those that do not. To get rid of needless nouns may sometimes take radical recasting, as in the following example, cited on page 524:

> . . . rats have an *ability* to make *selections* conducive to their *well-being.*
> . . . rats instinctively select the foods that are good for them.

Chains of prepositional phrases (p. 524) are likely to contain abstract nouns, and those that do can be broken up by cleaning out what the Fowlers call "noun rubbish" (*The King's English,* p. 15). That is what they did in radically recasting two examples on page 525.

The advice that I have summarized as precept (ii) appeared to me, at first reading, to mean that Struck would prefer that we never use the inverted constructions beginning with *it is, there are,* and so on (p. 523). He has been so kind as to point out, through the Editor, that he remarked a little further on that none of the constructions he "cudgels" must be condemned automatically, and that he used a *there are* on the same page. Many readers, however, might overlook that isolated *there are* and be less impressed by that generalized reservation than by the vigorous cudgeling of *there are,* and so forth.

There is good reason for the fact that these inversions, even though often used ineptly, are firmly imbedded in English idiom. They often make for effective placing of emphasis. In the second sentence back, for example, the uninverted form would be: "A good reason [16 words] is"—or "exists." That would throw a violent emphasis on *is* (or *exists*) and also is grotesquely unidiomatic. Of course Professor Struck knows all this, but it would have been helpful for him to say it explicitly.

In precept (iii), Struck is not so hard on the passive voice as he appeared to be on those inversions, but it would have been helpful to point out that the passive is sometimes better than the active, and to show why this is so. Often, of course, the passive does take a word or two more than the active, and when unskillfully used it is weak. But, like *there are,* and so forth, it sometimes makes for good placing of emphasis and good connection. In describing a stratigraphic sequence in ascending order, it is better to write "The Jefferson dolomite is overlain by the Madison limestone" than "The Madison limestone overlies the Jefferson dolomite."

The article fails to point out that words can be saved in ways that have nothing to do with verbs. In the longest example on page 524, many are wasted by elegant variation, which is carried over into Struck's rewrite. The reader's time is wasted, also, in figuring out that "rats," "animals," "rats," "animals," "rats," and "experimental animals," all mean "rats," and that there were seven of them. This secret is divulged in the first sentence of the following version, which con-

tains 57 words as compared with 89 in the original and 79 in Struck's rewrite. I took over his changes of passive to active voice but retained the passive, as he did, in the first sentence.

> Several weeks after these symptoms developed, the 7 rats were put on the self-selection diet. They then showed a marked appetite for fat, olive oil, and yeast, and little or none for the carbohydrate (sucrose). In all of them the diabetic symptoms disappeared or greatly decreased, but in 4 they reappeared when the McCollum diet was resumed.

Some words were saved here by using pronouns (*they, them*) and by ellipsis (*4 of the seven experimental animals,* for instance, was replaced by *4*). Both pronouns and ellipses are great word-savers. And the words they save are chiefly nouns; at least this is true of pronouns. The best way to save words is to get rid of expendable nouns—concrete as well as abstract. By doing this, one can save words of other kinds, including some of those weak verbs.

With this idea prominently in mind I rewrote the long example on page 522. Here all the nouns I removed entirely were abstract: *offer, exists, entrances, factor, control, case, study, fact, and practice*; but I also cut down the number of *scientists* from five to one. The original contained 159 words, Struck's rewrite 137, and mine 115.

Padding is bad, but the shortest way of stating a fact is not always the best way, especially when saving a word or two impairs continuity—the quality that makes one thing lead naturally to another—and cadence, which consists mainly in effective distribution of emphasis. The beginning of a sentence ought to connect well with the one that precedes it and to indicate promptly the general drift of what is to follow; the end should as a rule contain the most emphatic words.

Struck has perhaps not always borne the value of these qualities in mind. In the rewrite near the beginning of page 523, the first part of the last sentence is jerky, and the drift is not clear until we get halfway along; the best part is the last, which retains the strong ending of the original. In an example on page 524, shifting from passive to active changes a strong ending ("it is surrounded by great industrial regions") to a weak one ("great industrial regions surround it"). This saves two short words; but by so rewriting the sentence that it has good cadence throughout, one can reduce the wordage from 26 to 21:

> It is rather remarkable that there is so little industry in this area, surrounded as it is by great industrial regions.

I believe, after long experience in revising manuscripts, that that result is typical; good continuity and cadence cost a few words here and there, but they save words in the long run. So the reducing diet that I would prescribe is this: (i) improve the continuity and cadence; (ii) get rid of all needless nouns—which will automatically raise the percentage of "efficient verbs."

FRANK C. CALKINS

6431 Western Avenue, Washington 15, D.C.

13 July 1954.

Mr. Calkins and I are clearly brothers under the skin. But we prefer to go home by different ways. He is a noun-pronoun-ellipsis man and I am a verb man. The difference is probably not earth-shaking.

Looking for abstract nouns first, for instance, may get rid of weak verbs more efficiently than examining verbs first. I don't think so. I am inclined to think that looking for *tion* nouns (without worrying about their abstractness) is somewhat more valuable. Abstraction, however, is a concept often frustratingly elusive. Is *tree* concrete or abstract in "I think that I shall never see a poem lovely as a tree"? What about the nouns in "They take the logical position that improved working conditions for all workers will have a beneficial effect on the union membership"? I'd hate to be dogmatic about any of the nouns here, though I would probably run little risk in calling *effect* abstract and *workers* concrete. In short, the idea of abstraction isn't easy; it frightens some people, including me. I'd just as soon bypass it. I find that a writer, dissatisfied with a sentence, can spot *is, are, was, has, had, take, give,* and so forth, more readily than he can decide what is abstract and what isn't. If abstract nouns would only label themselves as clearly as the verb *to be,* I might change my mind. But until *-tion, -al, -ment, -ence, . . .* point the way as effectively as I find a relatively few verbs do, I'm staying a verb man. The interested reader can choose either system—or any other—as long as it helps him unpad his sentences.

While Calkins' revisions are consistently excellent in themselves, his criticisms sometimes fail to recognize that emphasis and plain acceptability of an article occasionally make perfection in handling details undesirable. His "rats instinctively select the foods that are good for them" is, in my opinion, superior to the original "rats have an ability to make selections conducive to their well-being" and to my "rats can make selections." However, too much revising can obscure a point. Though *make selections* is, normally, weaker than *select,* I avoided this change lest it dim the value of changing *have an ability* to *can.* And Calkins' addition of *instinctively* might provoke protest. I understand psychologists instinctively avoid it.

My emphasis on the verb permits Calkins to outscore me 57 to 79 on the diabetic rats. I concede. And his means are effective, too. Still, knowing that inaccurate pronoun reference can wreck the whole meaning of a sentence more readily than a repeated noun can, I hesitate to endorse the pronoun-for-noun substitution unreservedly. (An authoress on nutrition, next door to me, is so miffed with pronouns that she will use them only at gun point.) But, with reservations, the pronoun is a useful tool; in the hands of a skilled workman it can contribute its share to such an excellent rewrite as Calkins' 115-word revision of an originally 159-word passage. Since this revision merits a wider publicity than my unkempt files will give it, here it is:

> Most of the scientists were able to work in places that suited them, and to get new jobs immediately if they left the old. Of the 155 who had held one job

8 years or more, all but 18 had been offered some other worthwhile job within that period. Of the 574 who quit, only 67 did it because they had to, and 28 of these had been employed on war projects that were terminated. Of the 670 who had accepted employment, all but 75 had some other opening. This was partly due, presumably, to their having shopped for new jobs while remaining in the old: they rarely left one without having lined up another.

Taking up Calkins' discussion of the passive, I should, first, admit that he showed thoroughness and perception in pouncing upon the *great industrial regions* sentence. With so many bullet-proof illustrations to choose from, I can only kick myself for having used this one.

Second, perhaps I do overstate the case against the passive; let's call it the shock treatment or the academic white lie. If an awareness of the passive gives a writer a handle for straightening out a stumbling sentence, I've accomplished my mission. Obviously many passives are pure in heart and indispensable: the doer of the action may be unknown or unimportant; the doer may be a string of nouns too long to precede an active verb effectively; a weak construction may be more appropriate than a forceful one—in being tactful, for example. I object not at all to these:

> The cathedral at Chartres, founded in the 4th century, was dedicated to the Virgin and Child.
>
> After the fall of Rome this area was occupied by the Eruli, Ostrogoths, Greeks, Longobards, Moslems, Greeks (again), Normans, and the forces of the Holy Roman Empire, in that order.
>
> This paper is poorly written.

Calkins' statement that "words can be saved in ways that have nothing to do with verbs" is certainly true. But surely he is aware that space is limited—particularly for an article on writing in a scientific journal.

While most of Calkins' points are worth making, I do not understand his strong defense of inversions, particularly *there is*. True, inversions are occasionally necessary, even invaluable. Changing the following sentence, for instance, would be idiomatic suicide: "After V-J day, there was a counter movement into normal civilian activities." Too, I may have slightly overstressed the evils of *it is*. But on *there is* I refuse to retreat. In fact, if I were to rewrite the article, I would flail the construction harder. Although, unfortunately, I don't go in for statistics, from my reading I would guess—conservatively—that one out of every four sentences that contain *there is* would be better without it. This passage from a pamphlet on technical writing shows its potential for messiness:

> Any survey of the deficiencies in a large group of manuscripts is likely to show that these cover a wide range. Nevertheless, there is evidence that in some areas, there is need for much improvement of papers, while in other areas there is less need for attention.

The final sentence, without *there is*:

> Nevertheless, evidence shows that papers need much improvement in some areas and less in others.

Calkins' objections that my approach was negative means only that I used bad examples rather than good ones. And purposely, since I hoped to reach primarily *Science* readers who write bad sentences, know it, and feel stymied in their attempts to improve them. Good examples are admittedly a first-rate technique, but, within the space available, I preferred bad examples. I wasn't brain-washing the reader; I was blackjacking him.

Nor am I a blind devotee of concision. If my article so implied, I repent—slightly. I'm not greatly concerned, because even if the article did give that impression, it will probably injure few people; overconcise writers are as hard to find as a golden needle in a haystack.

Finally, Calkins' guiding principles of tone, continuity, and emphasis are beyond reproach—and in his hands highly effective. But they're also highly abstract. My article tried to suggest specific devices that less skillful writers might find helpful in achieving the desired tone, continuity, and emphasis.

HERMAN R. STRUCK

English Department,
Michigan State College, East Lansing

21 September 1954

The Explosion of a Planet

The heterogeneous structure of the meteorites suggests that they might have originated in an environment of violence, such as the explosion of a former planet; but the opinion of astronomers has been that ". . . there is no known reason why a planet should explode" (*1*). Despite this adverse opinion, it is suggested that, if a planet similar to the earth approached within Roche's limit of a larger body, its tidal disruption would release forces that would cause the planet to explode. Although the two catastrophes would occur almost simultaneously, they would result from such different causes and with such different results that they will be described in sequence.

Tidal disruption. When a planet approaches within Roche's limit of a larger body, tidal forces, which are tensile, pull it apart into disks at right angles to the tidal forces. The distance from rift to rift would vary from a few hundred kilometers in the cold exterior rocks to a few meters in the hotter interior rocks, since the distance from rift to rift would vary inversely as the square of the strength of the material. Gravitation of the material in the planet would be neutralized in the direction of tidal forces, but it would not be affected at right angles to tidal tension. The width of the rifts would continually increase, and at the same time the change in direction of the tidal forces through over 90° would cause new rifts at increasingly greater angles to the original rifts until the planet would be crisscrossed with rifts of varying and increasing widths. In the meantime, the vertical walls of rifts through the crust, like the walls of a deep trench or a deep mine, would collapse at a depth where their

weight equaled the strength of the rock. Their collapse would squeeze hot plastic rock into the rifts and crush cold brittle rocks into fragments. The failure of these rocks would cause large chunks of the surface rocks to break off and fall into the widening rifts. The original size of the fragments of the crust would be measured in hundreds of kilometers, but their thickness, limited by the weakness of the hotter interior rocks, would be perhaps 25 km. The changing direction of the tidal forces would eventually break these large crustal fragments into sizes of the order of 50 km. When the weight of the materials at right angles to the tidal forces is considered, as well as the reduction in their tensile strength because of internal heat, the size of the fragments would vary from Jeffreys' (*2*) 200 to 400 km to fragments perhaps the size of sand or dust (*3*).

The materials in the fragments would be identical in composition and structure with the materials of the disrupted body, just as the fragments of crushed rock are identical chemically and physically with the rock from which they are broken.

Quasi vulcanism. If the planet is assumed to have been similar to the earth, its subsurface rocks would have contained gases, principally steam, dissolved under pressures that exceed 10,000 atm in terrestrial subsurface rocks. On the earth, when tension rifts through the crust penetrate deep enough, molten magmas well to the surface, often escaping with explosive violence. If the interior rocks of the disrupted planet were not hot enough to flow into the rifts, but were too weak to retain the rapidly expanding gases, the dissolved gases would expand with such terrific violence that they would hurl the fragmented rocks into the widening rifts and at the same time fill the rifts with gases under considerable pressure.

When tidal forces were great enough, the metal core itself would be violently torn apart. At the temperatures and pressures existing within the planet at this stage, the metals would be fluid. Sulfides, halides, other volatile salts, volatile metals, and possibly dissolved water and occluded gases would all expand with terrific violence, tearing apart the tidal fragments of the core. The expanding gases would hurl molten metal toward the surface of the planet, but the molten metal would be expected to collide with and adhere to or interpenetrate the fragmented rocks in their paths, for unlike gases, their paths could deviate only slightly from a straight line. In general, the metal fragments would be larger than the stones, since tidal tension would be less effective in the fragmentation of the denser metals, the back pressure of the expanding gases above would partially replace gravitational force, and the time interval of tidal disruption of the core would be shorter.

Conclusion. If the tidal disruption and fragmentation of a planet would cause it to explode, it would seem that the whole interior of the planet would become an enormous volcano, belching forth its substance through every tidal crack and at the same time tearing to pieces and nearly pulverizing all rocks hot enough to permit the escape of their dissolved gases. Such an explosion, detonated almost simultaneously throughout the entire planet, would be so much more disorderly than an ordinary explosion that any attempt to describe the results in detail is necessarily speculative.

(The late) P. S. PALMER
135 N.W. 60th Street, Miami 37, Florida

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1. H. N. Russell, R. S. Dugan, and J. Q. Stewart, *Astronomy* (Ginn, New York, rev. ed., 1945), p. 357.
2. H. Jeffreys, *Monthly Notices Roy. Astron. Soc.* **107**, 260 (1947).
3. P. S. Palmer, *Nature* **173**, 499 (1954).

28 June 1954.

This communication, which is based on a portion of Mr. Palmer's unpublished paper, "The origin of meteorites," was prepared by Katharine B. Palmer after the death of her husband in September 1953. Mr. Palmer's paper and the present communication have been the subject of some criticism, privately transmitted to the authors. But our advisors, who saw these critical comments, have pointed out that none is very specific, probably because of the difficulty or impossibility of giving a merit rating to a hypothesis that involves essentially qualitative reasoning—however, this can hardly be held as an argument against publication.

Some Comments on a New University

It was certainly a pleasure to read William Seifriz' thoughtful article [*Science* **120**, 87 (1954)]. Surely he will not lack applicants for positions in the new university. I doubt, however, whether under present conditions his goal can be realized.

Seifriz complains that science has become tough. But is not everything tough today? Why should science be exempted from the general trend of events? Young men who wish to make science their career are not taught the value of contemplative attitudes, of a fuller view of life, but instead are trained in graduate "trade" schools where their advancement must depend on their ability to get results, that is, answers to problems posed by their teachers or initiated by themselves. If such results lack significance in a deeper sense, it is hardly fair to blame the students or their preceptors. In investigative work it is just impossible to foresee where an idea will lead. Some ideas that appear brilliant at their conception unfortunately turn out to be dreams without any basis in fact and, therefore, have to be discarded; others that seemed of little value at first yield results significant beyond expectation because they happen to be in accord with the course of natural processes. Failures will outnumber successes; but, regardless of what he himself may think of the results he has obtained, the scientific worker feels he has to present them in order not to lose in competition with others. No doubt, this makes many scientific congresses a hodgepodge of trivia, as Seifriz describes

them. But should one blame the scientists, who believe they have to keep up with the times for the sake of prestige and financial rewards that go with it? Obviously, they are on the defensive and may be most unhappy about it. But what are they to do? How can they change matters?

Seifriz recognizes the root of our present difficulties in the completely erroneous, yet commonly accepted, notion that science is power. Power to do what? How can a knowledge of natural laws give man power, that is, some kind of arbitrary decision over the workings of nature? Has our understanding and subsequent application of thermonuclear reactions had the slightest effects on the laws governing them? The only such power that can be gained from science is, in fact, power over other men, and that is the evil the world is facing today. It forms the major obstacle, not only to the new university, but to all pursuits and aspirations of civilized people.

What can scientists do about it? Kirtley Mather says they have an obligation to interpret science to the layman [*Science* **119**, 299 (1954)]. Of course, they have, and that is exactly what they have always done. There have always been scientists because men had to relate themselves to the world around them in order to survive. Even in prehistoric communities there must have been those who observed nature, learned something about her order and regularity, which certainly was of considerable help to their fellow tribesmen. Their interpretation of natural events was, of course, based on the very limited knowledge they had gained. It, therefore, should not surprise us that they attributed to good spirits whatever in nature appeared favorable to their well-being, and to evil spirits what they considered harmful. Later world religions were also attempts at an interpretation of the cosmos and man's place in it. Today these religions have lost much of their effectiveness, because, as Seifriz states, "the appeal is purely emotional, the reasoning often biologically unsound, and the approach too sentimental." In short, in the way that they are stated, they cannot claim objective validity. That is why men's actions only too often belie their protestations of belief in a formal religion.

To my mind, the fundamental problem before us today can be stated briefly as follows: Do we have to accept as true a materialistic interpretation of the universe and of man? Or can we recognize in the workings of nature a basic rationality that is akin to our own reasoning power? If in all sincerity we must reach the conclusion that the facts of nature all favor the materialistic theory, then we should be candid enough to admit it. It would mean, of course, that reason is only man's special tool in the struggle for existence, making this struggle between men and groups of men ever fiercer as our globe becomes more and more crowded. This in turn would lead inevitably to increasing regimentation, whether on the Soviet or any other model, and it seems quite possible that eventually a full-fledged dictatorship would be set up by the will of the majority. However, if there is reason behind the multitude of natural phenomena that we have been studying—and I personally believe that the evidence in support of this assumption, which cannot be given here, is overwhelming—then there is no cause for gloom, for then we shall be able to use the laws of nature to create health and order in men's minds, and not just in their bodies as we have done in the past. From such disciplined minds, I have no doubt, there will emerge all the things that Seifriz and so many of us want: a culture that man will respect, an intelligent biological system of ethics, a new university, a civilized society.

May I then humbly suggest that men like Seifriz, Mather, Loewi, and many others get together in a truly scientific congress, not to exchange information about trivia, but to consider this one vital problem only. The conclusions reached by them will be of immeasurable value to their fellow men.

PAUL H. KOPPER
Department of Microbiology and Public Health, The Chicago Medical School, Chicago 12, Illinois

I agree with Dr. Kopper on all points except one. I feel that science is not only guilty but doubly so. I have used the word science to mean all academic learning, for we now have the social sciences and the philosophy of science. In my article I state that the layman has come to expect goodness from the church, justice from the state, and knowledge from the university, knowledge mellowed by understanding. In this, science and all academic learning have failed. A Polish gentleman at the Sorbonne said to me recently: "In my youth I worshipped science; in my old age I have come to detest it." What a pity to fail to see the beauty in something so magnificent because of the weakness of men.

Why do so many of us recommend Claude Bernard's *Leçons* for supplementary reading? It is because he gives us not only experiments and data, but also hypotheses, then more experiments and data, always ending in a thought, a speculation, in anticipation of the next experiment. War, the business world, and society may become tough, but this does not relieve science of its obligation any more than it relieves the church of its obligation.

WILLIAM SEIFRIZ
Botanical Laboratory, University of Pennsylvania

16 August 1954.

Raphael paints wisdom; Handel sings it, Phidias carves it, Shakespeare writes it, Wren builds it, Columbus sails it, Luther preaches it, Washington arms it, Watt mechanizes it.
—R. W. EMERSON.

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Meetings & Conferences

November

11–13. American Assoc. for the Surgery of Trauma, Atlantic City, N.J. (J. K. Stack, 700 N. Michigan Blvd., Chicago 11, Ill.)

12–14. American Assoc. of Medical Clinics, New Orleans, La. (A. H. Griep, 420 Cherry St., Evansville, Ind.)

12–Dec. 11. United Nations Educational, Scientific, and Cultural Organization, Montevideo, Uruguay. (UNESCO, 19 Ave. Kléber, Paris 16.)

14–19. American College of Surgeons, Atlantic City, N.J. (M. L. Mason, 40 E. Erie St., Chicago 11, Ill.)

15–17. National Conf. on Standards, 5th, New York City. (D. E. Denton, 70 E. 45 St., New York 17.)

17–19. American Meteorological Soc., Miami Beach, Fla. (K. C. Spengler, 3 Joy St., Boston 8, Mass.)

17–19. Southwestern Medical Assoc., El Paso, Tex. (C. C. Stapp, 800 Montana St., El Paso.)

18–19. National Assoc. of Corrosion Engineers, western region, Los Angeles, Calif. (J. G. Kerr, C. F. Braun & Co., 1000 S. Fremont Ave., Alhambra, Calif.)

18–19. Soc. of Exploration Geophysicists, 8th annual midwestern, Dallas, Tex. (R. C. Dunlap, 624 S. Cheyenne, Tulsa, Okla.)

18–20. Acoustical Soc. of America, semiannual, Austin, Tex. (W. Waterfall, 57 E. 55 St., New York 22.)

18–20. Symposium on Precision Electrical Measurements, Teddington, Eng. (Director, National Physical Laboratory, Teddington, Middlesex, Eng.)

19–20. Operations Research Soc. of America, Washington, D.C. (D. A. Katcher, 7100 Connecticut Ave., Chevy Chase 15, Md.)

19–30. International Cong. of Civil Engineers, 2nd, Caracas, Venezuela. (L. B. Diaz, Av. Principal de los Caobos, Apartado 2006, Caracas.)

25–26. Inst. of Metals, London, England. (S. C. Guillan 4, Grosvenor Gardens, London, S.W. 1.)

26–27. American Physical Soc., Chicago, Ill. (K. K. Darrow, Columbia Univ., New York 27.)

26–27. American Soc. of Animal Production, Chicago, Ill. (W. M. Beeson, Dept. of Animal Husbandry, Purdue Univ., Lafayette, Ind.)

26–27. Tennessee Acad. of Science, Nashville, Tenn. (I. H. Tipton, Physics Dept., Univ. of Tennessee, Knoxville.)

28–1. American Soc. of Refrigerating Engineers, Philadelphia, Pa. (J. I. Szabo, 40 W. 40 St., New York 18.)

28–3. American Soc. of Mechanical Engineers, annual, New York City. (O. B. Schier, II, 29 W. 39 St., New York 18.)

29–1. Assoc. of Military Surgeons, Washington, D.C. (R. R. Sayers, Armed Forces Inst. of Pathology, Washington 25.)

29–2. American Medical Assoc., clinical, Miami, Fla. (G. F. Lull, 535 Dearborn St., Chicago 10, Ill.)

30–2. Electronic Computer Clinic, 1st, New York, N.Y. (R. Rimbach, 845 Ridge Ave., Pittsburgh 12, Pa.)

December

1–3. Animal Care Panel, Chicago, Ill. (R. J. Flynn, Box 299, Lemont, Ill.)

1–3. American Rocket Soc., New York, N.Y. (ARS, 33 W. 39 St., New York 18.)

(See issue of 17 September for more comprehensive listings.)

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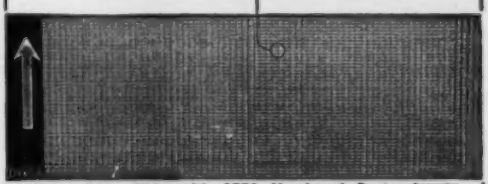

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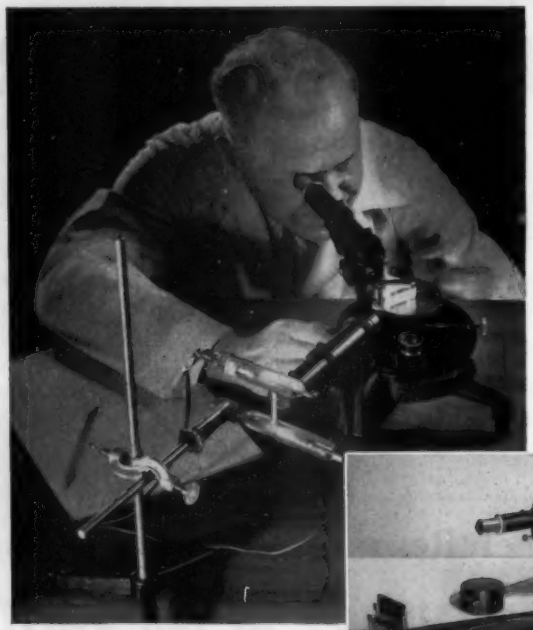

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